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Astronomy and Astro-Physics.

VOL. XIII, No. 3. MARCH, 1894. WHOLE No. 123.

General Astronomy.

PHOTOGRAPHIC NEBULOSITIES AND STAR CLUSTERS CONNECTED WITH THE MILKY WAY.

E. E. BARNARD.

That region of the Milky Way lying north and east of Orion, is singularly rich in large diffused nebulosities. I have lately secured a number of photographs of this part of the sky with the Willard lens (6 in. diameter, 31 in. focus.)

Some of these nebulosities were previously known to me, but they are so large and so diffused that their observation and location with a telescope is a matter of difficulty and uncertainty. They are, however, specially suitable for the photographic plate.

After the opening of this Observatory in 1888, while sweeping over this region, I found a very large, weak, diffused nebulosity some half a degree south of the nebula N. G. C. 2245. This was mixed up with several considerable stars. I also found a 10 mag. nebulous star about half a degree north preceding 2245.

In the photographs all these nebulæ are clearly shown, along with others not seen previously.

The large diffused nebulosity, mentioned, is shown very strongly on the plates, as an irregular elliptical mass about 36′ in diameter. It involves several considerable stars. DM + 10°.1159 and + 10°.1160 being respectively in the northern and southern parts of the nebula. Its position would be closely represented by the mean of the position of these stars, or for

$$1860.0 \quad 6^h\ 23^m\ 27^s + 10°\ 7'$$

close preceding DM + 10°.1159 is a singularly well defined vacancy, like a comma, in the nebulosity.

N. G. C. 2245 is shown as also is 2247—though from the small extent of nebulosity about this latter star it is but little blurred on the plate. Visually with the 12 in. the nebulosity is decided.

The 10 mag. nebulous star, before mentioned, is clearly shown, with a quite decided nebulosity. A small dark space exists in its nebulosity, also, south of the star. Position of this nebulous star is $1860.0 \quad 6^h\ 23^m\ 14^s \pm 3 \quad + 10°\ 32'.6 \pm$.

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The most remarkable object in this region, however, is the nebulosity connected with 15 Monoceros (this nebulosity is questioned in N. G. C).

This group of bright stars is mixed up with a knotted and wispy nebulosity extending faintly and irregularly north and westerly to an extent of nearly 2°. It is a very wonderful object and with a longer exposure—which I hope soon to give—promises to be one of the most remarkable nebulæ known. It is irregular in outline, but quite well defined, with numerous black gaps running into it, and in general conforming with the peculiarities of the Milky Way in that region, showing it to be actually mixed up with the stars of the Milky Way, resembling in this respect a large nebula I have recently found by photography in Cepheus. There is a sinuous, vacant strip in the Milky Way here that runs from 15 Monoceros, at first to the west for some 3°, and then northerly nearly to γ Gemini.

These remarks refer to a photograph made 1894, Jan. 24, with $2^h\ 30^m$ exposure with ξ Geminorum central. Another exposure, covering nearly the same region, of $3^h\ 35^m$, made Jan. 25, confirms the statements, but from the poor condition of the sky it scarcely showed more than the shorter exposure.

In *A. N.* 2918 I have given an account, along with a sketch, of a large nebulous ring enclosing the cluster G. C. 1420, the nebula itself being N. G. C. 2237. The place of G. C. 1420 for 1860.0 is $6^h\ 23^m\ 29^s + 5°\ 2'.5$.

The nebula (2237) was discovered by Swift very many years ago, and was independently found by me in January, 1883.

The sketch referred to was made with the 12-inch in 1889.

I have recently (Jan. 9, 1894) secured a fine photograph of this object with the Willard lens. As stated in *A. N.* 2918, from its diffused nature this nebula is specially suited for photography. This photograph verifies the statement most emphatically and shows how utterly impossible it is to adequately deal with such an object visually.

I will quote from my previous article on this subject in *A. N.* 2918, in speaking of its appearance with the 12-inch in 1889:

"What I had seen previously and what Swift had sketched, was simply a brightish knot in a vast nebulous ring that entirely surrounded the cluster. By estimation the average outer diameter of the ring is 40′ and the inner diameter 20′. The inside of the ring is apparently free of nebulousity, the stars of the cluster shining on a perfectly dark sky. The outer edge of the ring is somewhat diffused and irregular, some projections occuring near

the following portion. The inner edge is more definite and especially so following—it is less definite in the preceding part. In the north preceding section of the ring are several knots, the largest of which, *a*, is the one previously seen by Swift and myself. I am not sure that there is not a very small break in the continuity of the ring at the point *b*. South following the ring and close to it is the nebulous section of a large ellipse which seems to be a portion of another great ring; I am not sure that this is not connected with the first by a nebulous strip."

Comparing the photograph with my sketeh, I find the sketch is correct so far as it goes, but with the 12-inch I had grasped only the brighter details—the great mass of it not being seen in the 12-inch.

The photograph shows the nebula to be about 1° in diameter and very irregular in brightness and outline. It is a mass of unequally condensed nebulosity involving the star cluster and specially heavy north of the bright stars. The nebulous knots or condensations, shown in my sketch, are conspicuous on the photographs, as is also the "nebulous section of a large ellipse,' which is connected with the main mass; the full extent of this section is shown in the sketch.

The photograph shows that there is no nebulosity—or if any it is very feeble—immediately about the bright stars. They apparently shine in a vacant space in the south part of the nebula.

The entire nebula seems to be definitely terminated and to leave no suggestion of a greater extent being revealed through a prolonged exposure.

One degree south of the center of the nebula, and free of it, and following about ¼°, is a very thin nebulous strip 10′ or 12′ long extending north and south with a faint star in its south end, like a slender comet with a nucleus.

In one of these photographs north and east of Orion there is an extremely faint and large diffused glow near the stars ν and ξ Orionis. This extends south of ξ and acquires a slight density in about $6^h\ 5^m + 13°$. It is the most diffused glow of nebulosity I have yet seen on any of the photographs. It might almost be called the ghost of a nebula, so faint and vague is it.

I have now covered, photographically, a large portion of the Milky Way—from the Scorpion to Orion—securing characteristic photographs of the different regions. These pictures are very wonderful. They give us views not only beautiful but most intensely interesting and valuable. One thing very apparent from them is that the features of the Milky Way do not repeat them-

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selves and that the different regions have apparently a different order of structure as well as a different order of brightness of the stars. In one region the cloud forms will consist of coarse stars while in another they are made up of stars apparently comparable with dust particles. This may be due in the main to a greater distance from us in the one case. But I believe that many of these cloud forms are made up actually of comparatively small stars.

As we ascend from the south through the Scorpion, Sagittarius, etc., we find here and there nebulæ and compressed clusters scattered over the Milky Way, with nothing to imply any but an accidental connection of them with the Milky Way. But after entering Cygnus we come to a region in which vast masses of diffused nebulosity are present and are unquestionably actually mixed up with the ground work of stars. From this on as far as Monoceros we meet here and there with these affected areas.

These masses, however, are almost invariably mixed up with a group of stars brighter than the general average in that region. A magnificent specimen of these I have found on one of my plates in Cepheus which was given an exposure of seven hours. This is a mixture of bright stars and nebulosity. The diffused portion of this nebulosity conforms in its peculiarities with the general structure of the Milky Way, showing it to be actually mixed up with the ground work of stars.

The brightest star of this group is DM + 56°.2617, and its position for 1855.0, $21^h\ 34^m\ 29^s.8 + 56°\ 49'.7$.

These collections of nebulosity and stars are extremely interesting and suggestive taken in connection with the possible light they may throw upon the nebular theory. Indeed the loose clusters of bright stars can readily be separated into classes.

In the first there is no nebulosity mixed with the stars, such are the Hyades, the Dolphin, Præsepe in Cancer, M 11, the well known cluster in Perseus, etc.

In the second, stars and nebulosity are freely mixed together, such are the Pleiades, G. C., 1420, 15 Monoceros, M 8, the nebula in $6^h\ 23^m + 10°\ 7'$, the great nebula and cluster in Cepheus just referred to. G. C. 1366 is also of this class, as one of my plates shows it to be a mixture of bright stars and nebulosity. The nebulosity is about ½° in diameter. In the 12 in. the 8m star referred to in G. C. is found to be surrounded with a faint and small but decided nebulosity. In the finder (3¼-in.) the 8^m star is seen to be surrounded by a group of small stars—the whole being a loose cluster. The cluster is enveloped in feeble nebulosity. This shows strongly on the plate.

A splendid example of this admixture of stars and nebulosity is Wolf's great nebula near α Cygni, a fine photograph of which I have secured with 366 minutes' exposure.

If the nebular theory is true we have here the process of evolution readily illustrated—where a group of stars is being evolved from diffuse nebulous matter. According to this idea the second class of star clusters is still in an unfinished condition—the process of evolution still going on before us, while in the first class the work of Sun making is complete.

There is one point, however, and it may be an important one, where the Pleiades differ from the rest of these nebulous clusters. In its case the nebulosity is condensed about the individual stars, in nearly all the other clusters referred to the nebulosity does not seem to attach itself to any individual star but to simply involve the group, the stars themselves not showing any special tendency to condensation individually.

A photograph which I have made in 1892 with five hours exposure was χ Cygni, shows γ Cygni to be surrounded by numerous large patches and strips of nebulosity.

I have sent a list of other photographic nebulosities to *Knowledge* along with several photographs for reproduction, which will perhaps appear in the February or March number of that journal.

MT. HAMILTON, 1894, Feb. 1.

POSTSCRIPT.—On another photograph taken Feb. 1, 1894, with $2^h\ 10^m$ exposure, the $9^m.5$ star Dm $+ 23°.1313$ is found to be closely nebulous, a very small dense nebulosity gives it a fuzzy appearance. This nebulosity is heaviest south and following. The place of the star for 1885.0 is

$$\alpha = 6^h\ 11^m\ 35^s.8 \quad \delta = + 23°\ 19'.3.$$

On this same plate is a faint narrow curved nebulosity in about, 1860.0,

$$\alpha = 6^h\ 8^m \quad \delta = + 23°\ 0'$$

It is nearly ½° long, extending north and south and convex to the east.

The cluster M35, occupying the middle of the plate, does not show any evidence of nebulosity. This therefore is one of the non-nebulous clusters. It is however a very beautiful object.

The condensed cluster N. G. C. 2158, south preceding M35, also does not show any traces of nebulosity.

The nebulous star N. G. C. 2175 which is better shown on this plate than on previous ones, is quite a striking object. The nebulosity is about ½° in diameter, nearly circular, but irregular at the preceding edge. The 8 mag. star is in the center of it. The density of the nebulosity in its preceding part is somewhat irregular. Otherwise it is nearly uniform in its light. There are quite a number of considerable stars mixed up with it north.

Another photograph of the region about 15 Monoceros on February 1, 1894, with 3 hours' exposure, shows the great nebula that envelopes 15 Monoceros very much better than the previous picture. Its full extent—in diameter—can be taken roughly at 3°. It clusters densely about the groups of stars and then spreads out in a weak, diffuse light with rifts in it and irregularly terminated along the edges of a vast vacancy in the Milky Way. The condensation, which is very strong, is not at 15 Monoceros but 12′ south preceding that star, where it becomes a compact mass, with numerous wisps and holes in it. The whole group of three or four bright stars are involved in this denser wispy light, but 15 Monoceros itself does not seem to be specially connected with the nebulosity further than to be apparently in it—that is there are no indications of condensation about this the brightest star of the group. This remarkable nebula—the denser part of it—is worthy of study with a more powerful photographic telescope. The condensed part of the nebula is only a few minutes in diameter—but it would readily photograph in a large instrument. The place of 15 Monoceros for 1860.0 is $\alpha = 6^h\ 33^m\ 16^s + 10°\ 1'.3$. It is thus described in the catalogue (N. G. C.) "15 Monoceros Cl.:, ? neb."

During the exposure for this picture, at $10^h\ 6^m$–7^m Standard Pacific Time, the sky was suddenly illuminated, comparable to that from a full moon, this quickly increased for two or three seconds and then disappeared. Rushing quickly to the slit in the little dome, I examined the sky in all directions for some traces of a meteor, but no train could be seen. From the moment I saw the light until I was examining the sky did not exceed 10 seconds. This I knew must be the light from an enormous meteor, in the north or east, but as I was facing the south I saw only the illumination from it. Mr. Perrine, however, was more fortunate as he saw the meteor itself in the east. What struck me as remarkable was the sudden disappearance of all traces of the meteor.

On another photograph February 2, 1894, with 3 hours exposure, in the region of δ Cassiopeia, there are shown two very singular fan-shaped patches of nebulosity close to γ Cassiopeia. One is a little following and north of γ; the other slightly north and

following. Each is about 24′ distant. These are about 15′ in diameter and point nearly towards the star.

In *Knowledge* for January, 1894, p. 17, I have a paper on the subject of Photographic Nebulosities. At the close of that paper I have called attention to what is apparently a large diffused nebulosity about α Orionis.

I have there cautioned any one from accepting this as real until verified. The star falls near the edge of the plate and I have no means of telling if it is real or not. Unfortunately I have not yet been able to get another picture with exposure sufficient to decide if this is real. All the other nebulosities I have mentioned, are verified either by the telescopic observation or by another photograph. This, however, is not verified and possibly is not real.

February 3, 1894.

POSTSCRIPT No. 2.—Another photograph of the region about γ Cassiopeia on February 6 with 1 hour and 50 minutes exposure, again shows the two queer looking nebulæ near γ.

NEBULÆ NEAR γ CASSIOPEIA.

In the latter half of the exposure the sky became thick and finally stopped the work. Before beginning this exposure, I carefully examined the sky close to γ with the 12-inch and a power of 80, with a field of 42′. The sky was fine. It was with the utmost difficulty that I could see these two nebulæ. They were excessively dilute and faint, and never would have been detected if the photographic plate had not revealed them. They, however, photograph very readily, and I think would be shown with considerably less than an hour's exposure.

The enclosed sketch shows their position with reference to γ Cassiopeia. It is on twice the scale of the original negative.

February 7, 1894. E. E. BARNARD.

ELECTRICAL CLOCK CONNECTIONS FOR OPERATING THE CHRONOGRAPH.*

G. W. HOUGH.

The earliest employment of a clock for making automatic signals by the use of electricity, was by Wheatstone in 1841. The clock was made to make and break the circuit every minute, by means of a disk attached to the escapement wheel arbor. During the preliminary stages of the chronographic method for recording transits in 1848-9, there was considerable discussion regarding priority in the invention of an automatic connection for making a clock record its beats on a moving fillet of paper. It would appear from the evidence that several persons at about the same time independently solved the problem. Professor O. M. Mitchel, Director of the Cincinnati Observatory; Professor W. C. Bond, Director of the Harvard Observatory; Dr. John Locke, Cincinnati, and Joseph Saxton, Esq., U. S. Coast Survey, each devised a clock connection.

O. M. Mitchel used a light wire cross supported on a horizontal axis and connected with the bottom of the pendulum by means of a spider's web. At every alternate oscillation of the pendulum one arm of the cross dipped in a cup of mercury, thereby completing the electric circuit. In my use of this connection at the Cincinnati Observatory in 1859, considerable trouble was experienced from the breaking of the spider's web. I soon found that the apparatus was just as effective by dispensing with the web and allowing the pendulum to strike the arm of the cross and tilt it enough to complete the circuit. The apparatus in this form I subsequently used for a number of years at the Dudley Observatory. The friction required to be overcome for operating the cross, materially affected the rate of the clock. If the clock was rated to run free, when the connection was installed, the rate was changed about two seconds daily.

W. C. Bond used a light flat spring six inches in length, terminating in a platinum point. The spring was bent to form a "V" at the center, and was mounted horziontally near the middle of the pendulum rod. By means of a pin secured to the pendulum, at every oscillation, the platinum point was lifted out of a mercury cup in which it rested, thereby breaking the circuit.

Dr. John Locke used a tilt-hammer struck by the teeth of the escapment wheel (solid connection).

* Communicated by the author.

Joseph Saxton arranged a tilt-hammer, which was struck by a piece of glass projecting from the middle of the pendulum (solid connection).

In 1851, the U. S. Naval Observatory used a clock by Frodsham, arranged with two knife edges at the bottom of the pendulum, which simultaneously passed through separate globules of mercury. I am not informed who devised this form of clock connection.

During the early experiments with clock connections, it was thought that it would be injurious to the clock to allow the electric circuit to pass through it, and hence the aim was to construct a connection which would not be open to this objection.

At Greenwich in 1854, contact springs pressed together by the pendulum at every oscillation, were used, but as the clock rate was not satisfactory, the method was changed. Later, the connection was made by means of contact-springs operated by a wheel of 60 teeth placed on the escapement shaft.

The employment of a single globule of mercury at the bottom of the pendulum, according to Chauvenet, was suggested by Joseph Saxton, U. S. Coast Survey.

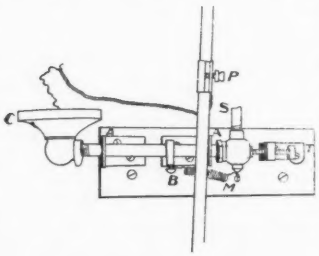
At an early date a break-circuit was devised in Germany, in which two reservoirs of mercury, having capillary tubes inserted in the side, were employed. The ends of the capillary tubes were brought so nearly together that the mercury formed a bridge between them. A plate of mica, attached to the pendulum, cut the thread of mercury at every oscillation, thereby breaking the circuit.

In 1860, Dr. F. Brunnow used a light bar magnet about two inches in length, which was pivoted at the bottom and set in a vertical position. When this apparatus was placed in front of the pendulum at the center of oscillation, the bar needle was attracted by the steel mercury jar and the magnetic contact was broken every second. Dr. Brunnow found that in order to make it work well, it was necessary to use a very weak battery and a relay. I used this connection for a short time in 1860 at the Dudley Observatory. The description of the apparatus will be found in Brunnow's Astronomical Notices, June, 1860. This apparatus is entirely disconnected from the clock, and when used with a weak battery, has a very slight effect on the rate. If, however, it is used in an ordinary circuit, there would be sticking between the platinum surfaces, requiring a more powerful magnet, and hence, absorbing so much more energy.

From 1867 to 1870, for operating the O. M. Mitchell disk

chronograph, I used a wire arm about 4 inches in length attached to the pendulum at right angles near the point of suspension. A fine platinum wire at the end of the arm dipped vertically in a cup of mercury at every alternate oscillation. The effect of this connection on the clock's rate was practicaily *nil*, but one cannot use it with the ordinary chronograph because the circuit is closed for too long a time. This connection was used for two consecutive years without being disturbed.

In 1870, I adopted the then well-known method of causing a platinum wire attached to the pendulum, to pass through a globule of mercury placed at the bottom, or preferably near the middle of the pendulum. Instead, however, of using an isolated column of mercury, which requires frequent renewal and is *not* constant in its effects on the clock rate, I used a siphon communicating with a large reservoir of mercury, thereby keeping the globule of mercury constant for a year or more without attention. A connection of this kind was used on a clock at the World's Columbian Exposition, for operating the Printing Chronograph. And the foreign astronomers who saw it were especially interested in its examination. As a description has never been published, a working drawing showing its construction in detail may be of value.



Clock Correction.

In the diagram, *p*, platinum wire attached to the pendulum by means of a collar and set screw, so that it may be set at the proper height, and also turned to bring the wire over the centre of the globule of mercury. *S*, an iron siphon made of ⅜-inch gas pipe and inserted in the bottom of the mercury cup *C*. The cup *C* is made of hard wood and may have a diameter of two or more inches. The siphon is supported at *a* on a block of hard wood and mounted, so that it may freely slide in the direction of its length. The collar *b* has a pin on the under side which works in a slot. By means of the set screw in the collar *b*, the siphon may be adjusted in a vertical position. A milled head screw *t* presses against the siphon leg, and a spiral spring *m* holds the siphon firmly against the screw *t*. The object of the adjusting screw *t* is to be able to make the seconds equal without stopping or interfering with the clock. When the connection is set up, the quickest and most accurate adjustment is made by allowing the pendulum to swing,

so that the platinum wire will just pass over the mercury globule and break circuit equally on each side. If this adjustment is properly made, the connection will operate a year or more without attention. By having a surface of mercury of the dimensions specified, the loss from oxidation will not lower the level of the globule an appreciable amount for a long period.

When I used this apparatus at the bottom of the pendulum, the siphon was fastened to a flat iron bar of the same length as the pendulum, in order to compensate for the effect of temperature. But when used at the middle of the pendulum, it is not of so much importance. In all forms of mercury connections, however, if impure mercury is used, the oxide may sometimes adhere to the point of the platinum wire, in which case the circuit will not be established for some seconds after being closed. A drop of kerosene oil placed on the globule will usually eliminate this source of trouble. If, however, one uses pure mercury and not less than seven cells of gravity battery, the connection is no more liable to fail than if made between platinum surfaces.

Recently I have employed the gravity cells in an indirect manner. Seven gravity cells are joined in series with three small storage cells, and the current for operating the chronograph derived from the latter. By this plan, one gets nearly the same voltage and a considerable increase in the amperes, provided the circuit is of low resistance. Gravity cells used in this manner, will run six months without cleaning; it is necessary, however, to add water and sulphate of copper from time to time.

The connection which I use when operated by a twelve pound pendulum, describing a semi-arc of 1°.5, changes the normal rate of the clock 0.11 sec. daily, and the semi-arc of oscillation is shortened about 0°.03. If applied to a heavier pendulum, the effect would be still less in distributing the normal rate.

In all the methods hitherto devised, however, for making a clock record automatically, more or less work is required to be done, which is prejudicial to the maintenance of a uniform rate. The chief objection to the solid connection, lies in the fact that too much work is required to be done by the clock. As friction is never constant, it should be reduced to a minimum to get the best results.

METEORIC ASTRONOMY.

DANIEL KIRKWOOD.

III.

The Andromedes, or Biela Meteors.

Another swarm of meteors, now well known, is that produced by the dissolution of Biela's comet. The story of this wonderful body, its separation into two parts in 1845, its further breaking up into fragments so small as to be separately invisible, except when rendered luminous by coming in collision with the atmosphere of the Earth—all are now matters of familiar history.

The two parts into which Biela's comet separated had, before passing out of sight in 1846, attained a distance from each other of more than 200,000 miles. This distance before the next perihelion passage in 1852 had become 1,500,000 miles, and before another period had been completed, the fragments, by further division, had become invisible. In 1872, however, the separated parts were seen as a meteoric shower. The phenomenon was again repeated in 1885, when, during the display, a meteoric stone of ten pounds weight, fell at Mazapil, in northern Mexico. This interesting object is therefore regarded by some astronomers as an actual part of Biela's comet. The last shower from this cluster, November 23, 1892, was very generally observed throughout the United States. It was thus noticed by the *Scientific American* of December 17th:

"Persons who happened to be in the open air on Wednesday evening, November 23, had the privilege of witnessing a phenomenon of more than ordinary interest. A brilliant display of celestial fireworks commenced about six o'clock and lasted several hours. Meteors at the rate of several hundred per hour were watched and counted by numerous spectators. The writer, at early twilight, counted 150 meteors in thirty minutes. Later, a neighbor made the number 700 per hour, and the whole number seen at a single station during the display must have amounted to thousands. How are the phenomena to be accounted for? How frequently do they occur? And when may they be again expected? were questions asked by many observers during the display in southern California.

"Aged persons remember Biela's comet—a telescopic body having a period of six years and eight months, or three periods in twenty years. One of its returns was due in the latter part of 1845. Instead of appearing alone, as on former returns, it was

seen as two separate bodies, as far apart as the Moon and the Earth. The dissolution of this wonderful body had therefore commenced, and the return (in 1852) was accordingly looked for with still increasing interest. It came true to time, but the fragments still further apart. That was the last time it ever appeared *as a comet.* It was due in 1859, 1865, or 1866, 1872, 1878, 1885, and in November, 1892. This process of falling to pieces began, as we have seen, in 1845, and advanced from year to year till the fragments became too small to be individually seen. They pass, however, through our atmosphere, become ignited, and are thus rendered visible. In a work entitled "*Comets and Meteors,*" published several years since, it was said of this shower's predecessor: "This cometary mass will be in close proximity to the Earth about the last of November, 1892. Another brilliant meteoric shower may therefore be expected at that epoch."* This is the shower just seen, as predicted in 1873. The study of these periodic showers has established many facts in regard to their phenomena—facts now to be found in recent works on astronomy.

At Leland Stanford, Jr., University, Palo Alto, California, the meteors were observed by Professor W. J. Hussey. A single person could see on an average from 50 to 60 fairly bright meteors every five minutes, "corresponding to a daily rate of 400,000,000 to 500,000,000 on the hemisphere turned to the radiant." Mr. C. D. Perrine of San Francisco, observed from $7^h\ 32^m$ to $8^h\ 50^m$, and in this interval of $1^h\ 18^m$ counted one thousand and thirteen. "An observation of a few moments again at $10^h\ 15^m$ showed them seemingly as frequent as before."†

From different parts of the country the present writer received newspaper reports, as well as MS descriptions. A somewhat careful comparison gave 300,000,000 as an approximate estimate of the whole number visible during the fall of six hours. Efforts of this nature, however, may yet be regarded as premature.

Of the several star showers now well known, the Andromedes afford the best opportunity for satisfactory study. The Leonids visit us but once in 33 years. The Perseids have become so widely diffused around the orbit as to be regarded annual visitors; but with their spread around the pathway of the comet their simultaneous numbers have become inconsiderable. The Andromedes appear in large and interesting swarms; the period is between six and seven years. The life of Biela's comet was but 80 years

* Comets and Meteors, p. 88.
† Pub. A. S. P., Vol. IV, p. 255.

—from 1772 to 1852. The very interesting discovery of the relation between comets and meteors had not been made when the dissolution of Biela's comet was announced in 1846. It may not be wise to anticipate future developments. Whether the present condition of the August stream indicates decadence—the final dissipation of its constitutional parts, as well as that of other streams are questions which may be left for future astronomers. The sporadic shooting stars observed on every clear night may be but fragmentary remains of meteor streams long since extinct.

History.—This interesting body was discovered by Montaigue, at Limoges, March 8, 1772. At its second known apparition it was seen by Pons, November 10, 1805. Its periodicity, however, was not discovered till after its third appearance, when seen by Biela, Eebruary 27, 1826. At this return its elliptic motion was demonstrated. Its first *predicted* apparition—the fourth observed,—was in 1832, when it was detected by a French astronomer. The fact of its crossing the Earth's path in November of that year having been misunderstood by the illiterate public, created temporary alarm. Its position in 1839 was unfavorable for observation and it escaped detection. In 1845-6 occurred the well known and often described division into two parts, and in 1852, its last return *as a comet*. The meteoric visits have been already described, at least those from 1872 to the present time. It is now known, however, that the comet had been in the process of dissolution before 1845. The meteoric phenomena of 1838 and of 1798 were undoubtedly derived from this source.* In short, the facts as now known, indicate that several cometary or meteoric bodies have, since 1772, or perhaps for some time before that epoch, been moving nearly in the path of Biela's comet. The meteors seen by Brandes in 1798 as well as those reported by E. C. Herrick December 5-7, 1838; also the body detected through the agency of Klinkerfues in Germany, and Pogson in Madras, in 1872,—all doubtless belong to the same cluster. Its future phenomena will be watched with interest.

THE SMALL NEBULA AT M. 57.†

EUGEN VON GOTHARD.

In reference to the small nebula found by E. E. Barnard of the Lick Observatory near the Ring Nebula of Lyra, I beg to say

* See the catalogues of Quetelet and E. C. Herrick. Also Newton's lecture "The Story of Biela's Comet," *Am. Journal of Sci.*, February, 1886.

† Translated by Miss Madelaine Hummel.

that it has been photographed by me years ago; it can be seen on some of my plates, but it is so small and looks so much like the surrounding stars, that it was impossible to identify it.

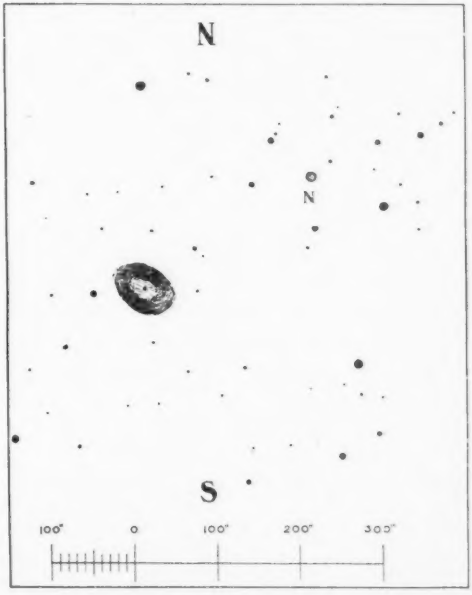
Of the numerous photographs, which I have taken of the Ring Nebula in order to study the small central star, which I discovered by means of photography, I have chosen those that have been exposed the longest, such as:

I. 1888, June 13, $9^h\,50^m - 11^h\,50^m$; Exp. = 2 hours.

II. 1891, Sept. { 2, $10^h\ 7^m - 11^h\ 7^m$ } " 3 "
{ 3, $9^h\,20^m - 11^h\,20^m$ }

III. 1891, Sept. { 8, $9^h\ 5^m - 12^h\ 5^m$ } " 6 "
{ 9, $8^h\,55^m - 11^h\,55^m$ }

I have measured in the Plate I, which shows the most sharply defined pictures, the coördinates (α and δ) of the stars, and drawn



enlarged to twenty times the original the accompanying sketch, comparing it with Plate III, and perfecting it, so as to show all the stars that can be seen on Plate III. The nebula is marked N.

I found the distance of the nebula from the central star of the Ring Nebula in Plate III to be as follows:

Dist. = 2.316 millimeters = 245″.6
Pos. Angle = 305°

Unfortunately the Ring Nebula has been so much over exposed, that the star can hardly be seen, which greatly diminishes the correctness of the measurements; on the other plates the nebula is too faint for any measurement.

Considering the coincidence with the results given by Mr. Barnard,* I think I may take it for granted, that I have not mistaken some other object for the nebula, but that it really is on my plates.

On this occasion I must again refer to the inestimable advantages of stellar photography by means of which I was enabled to confirm those observations which have been made with the best, the largest and the most favorably situated instrument in the world.

HERENY ASTRO-PHYSICAL OBSERVATORY.
January 24, 1894.

A PHOTOGRAPH OF THE PLEIADES AND TWO ASTEROIDS.

H. C. WILSON.

A photographic plate was exposed to the Pleiades for four hours on the night of Jan. 30, by the writer at Goodsell Observatory. The telescope used has an 8-inch objective with three lenses by the Clarks. It was driven by clock-work and the errors produced by irregular driving and refraction were corrected every minute or two by the observer, who was looking through a 3-inch finder at the star Alcyone, keeping it continually bisected by two cross-wires. As a result a very fine picture was obtained of the nebula involving nearly the whole group of bright stars and exhibiting marvelous details of structure resembling those of the great nebula of Orion. The curious straight lines of nebulosity running in some cases from star to star are shown, but are not quite so narrow and hard-edged as shown in the reproductions of previous photographs. The connection of the nebula with the brighter stars of the Pleiades is so obvious that one could hardly doubt it after inspecting the photograph.

All the star images are round and well defined, the very faintest star visible in our 16-inch telescope being easily seen on the plate, while many more can be made out with a magnifying glass.

* *Astronomiche Nachrichten*, No. 3200. Dist. 242″.8. Pos. Angle 303°.3.

The structure of the nebula about Merope is very curious. There are many approximately parallel and slightly curved lines of light passing the star at an angle of about 30° to the meridian. The same structure is apparent, though less marked, in the diffuse part of the nebula which extend to the south and east of Merope. From the southern edge of the image of electra a bright streak of nebulosity about 20″ in width, proceeds on a straight line toward Alcyone. It extends about one-third of the way to Alcyone, tapering to a point. About 1′ south of this a narrower parallel streak extends half the distance of the former, and 5′ south another streak, almost as bright as the first and nearly parallel, extends from a point 2′ west of Electra to a point 5′ or 6′ northwest of Merope, taking in on the way the two brightest stars between Electra and Merope. Through the middle of the group and especially around Alcyone the background is filled with mottled patches of nebula. A series of elongated patches form a line extending from the star almost midway between Alcyone and Maia eastward, joining a curved row of stars, almost to the limits of the group.

The region about Maia is especially interesting. A very bright hornshaped patch of nebula runs out from the west edge of the star image curving immediately northward, and extends to a distance of 3′ north of the star. The nebula here is full of irregularly parallel streaks similar to those about Merope but making only a very small angle with the meridian. Some of them run to and beyond the bright stars north of Maia. A series of rather broad and diffuse patches extend from the middle of the group on a diagonal toward the northwest, reaching to a pair of comparatively bright stars in that direction.

All these features agree very closely with those in the reproduction of a photograph by Mr. Isaac Roberts, given in *Knowledge* May, 1891.

Among the stars on the plate were found two straight lines each about 1.2 mm. long, having, aside from their length, the same structure as the star images. These were at once suspected to be minor planets which, because of their motion during the four hours of exposure, impressed lines instead of round dots upon the plate. When these were examined under the microscope a gap was found in the middle of each trail corresponding to the five minutes in the middle of the exposure, when the plate was covered in order that the driving clock might be wound and the telescope readjusted. This completely verified the supposition that the lines were the trails of minor planets. The same aster-

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oids were photographed again, one of them on Feb. 1 and both on Feb. 3. We have measured their approximate positions from the three photographs obtaining the following results:

Asteroid.	Central Time.			R. A.			Decl.		
		h	m	h	m	s	°	′	″
a.	Jan. 30	9	22	3	38	13.2	+ 23	27	49
	Feb. 1	7	32	3	39	27.5	+ 23	28	57
	Feb. 3	8	30	3	40	51.6	+ 23	30	38
b.	Jan. 30	9	22	3	41	24.6	+ 24	50	29
	Feb. 3	8	30	3	44	17.0	+ 24	54	43

The brightness of an asteroid is somewhat difficult to estimate from the trail, since the rate of its motion as well as the duration of exposure enters as a factor into the intensity of the trail. As an approximation we may divide the trail into parts equal in length to the diameter of star images of the same intensity. The ratio of brightness will be the number of parts thus obtained. It remains then to determine the brightness of the stars thus used for comparison.

In the present case the asteroid trail *a* was found to be equal to ten stars whose magnitude was estimated to be 14 on Argelander's scale. This according to the usual formula would give the magnitude of *a* as $14 - 2.5 \log 10 = 11.5$. Asteroid *b* was found to be equal to twelve stars of the fifteenth magnitude, its resulting magnitude being $15 - 2.5 \log 12 = 12.3$.

The identification of an asteroid in the list of nearly four hundred is something of a task unless an accurate ephemeris happens to have been computed for that particular one for the time of the observation. A large number of such ephemerides is published in the *Berliner Jahrbuch*. Each covers, however, only one month near the time when the planet is at opposition, and in the present case the region photographed was not opposite the Sun. Another table in the *Jahrbuch* gives the time of opposition, and the right ascension and declination at that time, of each minor planet. A little study of this table will generally enable the observer to exclude all but five or six of the known asteroids as too far from the given region. For the remaining number it is necessary to calculate the latitude and longitude or right ascension and declination of each from the elements of its orbit, in order to compare them with the same coordinates measured from the photograph. Where the elements have been brought up to date, this process is not so very difficult, but when the elements given belong to an epoch several years back it is necessary to calculate the perturbations of the orbits by the large planets, a process involving much labor.

In this instance the asteroids Nos. (33), (184), (196), (203), (207), (235) and (309) were found by inspection to be somewhere in the vicinity of the region of sky photographed. The calculation of the latitudes and longitudes, however, showed that only (203) was within the region of the photograph, its place falling within 3^m of longitude and 5′ of latitude of that of *a*. The two were therefore assumed to be the same. The asteroid *b* is probably a new one, although it is possible that perturbations which were not allowed for in reducing the elements of (207) and (309) from 1889 and 1890 respectively to 1894 may have sufficiently changed their orbits to bring one or the other of them into the place of *b*.

Since the above was written a telegram has been received from Berlin *Rechen-institut*, to which the observations were referred, giving the following ephemeris of *b* from circular elements determined by Berberich. We infer from this that the planet is a new one and that further observations are desirable.

Greenwich M. T.	R. A. h m s	Decl. ° ′
Feb. 23.5	4 04 28	+ 25 30
27.5	4 09 28	+ 25 38
Mar. 3.5	4 14 48	+ 25 48

TWO NEW VARIABLE STARS.*

M. FLEMING.

A recent examination of the photographs of stellar spectra forming part of the Henry Draper Memorial work at the Harvard College Observatory, Cambridge, has led to the discovery that the stars A.G.C. 157 in R.A. $0^h\ 10^m.4$, Dec. $-32°\ 36'$, Magn. 8, and B.D. $+1°\ 3417$ in R.A. $17^h\ 14^m.5$, Dec. $+1°\ 37'$, Magn. 9.5 are variable. The first named star is in the constellation Sculptor and its magnitude varies from 6.5 to 10. The second is in the constellation Ophiuchus and it varies from the magnitude 8.5 to 12.5. The approximate positions for 1900 are those given above.

HARVARD COLLEGE OBSERVATORY,
Cambridge, Mass., Feb. 13, 1894.

* Communicated by Edward C. Pickering, Director of Harvard College Observatory.

COMETARY SHOOTING STARS*

HUBERT A. NEWTON.

I have to apologize somewhat in that I came to the rooms not expecting to speak to you. I have, however, one point which I think will interest the members of this Society if they will give me a few minutes to develop it, and that is the force which acts on the small bodies sent off from comets and which form our shooting stars.

There are in the comets so many questions that we cannot answer, so many curious and wonderful phenomena that are unexplained, that I am sure you will accept my explanation of any of them that seems plausible, as a matter of interest. From a comet there is continually driven off matter forming the tail, a light substance, and astronomers are agreed that the force that acts on the matter which forms the tail is a repulsive force from the Sun acting inversely as the square of the distance, the force of the repulsion being greater than that of attraction.

Not only is this true, but different parts of that tail are acted upon by repulsive forces of different powers; otherwise the tail would form across the sky a single line intead of a broad, expanded mass of light such as we see. From the comet, however, there are driven off also, or there are separated, other things entirely distinct from the tail, small bodies, which are not thus driven away, which are not visible, but follow along closely in the path of the comet, and whenever the occasion comes, that is, when we go through a group of them, those give us our shooting stars.

The Biela comet, in the period about 1840, passed near to Jupiter. At that time it was turned pretty sharply out of its orbit, the inclination of the orbit being turned several degrees, and the node being carried forward also several degrees, represented by several days in the time at which we crossed the path of the comet.

After 1840 the bodies which formed the meteors that were met in 1872 and in 1885 were separated from one or other parts of the Biela's comet. I say after 1840, because if they had been separated earlier they would have given us a different radiant in the skies, the one given by the Biela meteors of 1838. The radiant was changed, the node was changed, all to correspond to the new

* Address reprinted Nov. 22, 1893, from Proceedings *Amer. Philos. Soc.*, Vol. XXXV., as corrected by the author.

orbit, and these bodies could not have been turned in that way had they been before scattered, because the force that acted on them, the attraction of Jupiter, would have scattered the group instead of giving us that single compact group through which we passed in 1872 and 1885 in the course of four or five hours, and the bulk of them in even two hours.

In 1872, the comet was something like 200,000,000 miles away from the bodies that we met as we passed through them on the 27th of November, giving us a brilliant shower. Thirteen years later we passed through the group again and then we were something like 300,000,000 miles ahead of the comet. So that some of the particles leaving the comet between 1840 and 1870, had fallen behind and others between 1840 and 1885 had gained.

What should separate those particles? What are the forces which carried off those particles so many miles—200,000,000 miles on the one hand and 300,000,000 miles on the other, in round numbers? The force that acts on them must be a force acting in one plane, that is, the plane of the orbit of the comet.

Any force acting in other planes would have scattered the group and we would not have met them as a single definite group at the times named; but if it acts in the plane, only scattering them on the plane, they would be together as we saw them.

In that plane, it must be either an impulsive force acting once or it must be a constant force acting continually. The only bodies in that plane are the comet and the Sun, and if the force is a continuous force it must be from the comet or from the Sun. It is almost inconceivable to suppose that the comet could have sent them off, either impulsively or continuously, in such a way as to give us the distance of 200,000,000 and 300,000,000 miles in the course of thirty years; it would require far more than any velocity that we can give in our terrestrial experiments, and we have no reason to suppose that there is any such power of impulsion. Moreover, if the impulsion came from the comet, they would go in all directions and their character, as being in a plane, would have been entirely lost.

We are then thrown back on this one hypothesis, that the Sun is the source of that force. In other words, we are led to extend the idea that I gave you in the beginning, and which is accepted by astronomers, that the material which goes off from the comet, after it leaves it, is subject to a force like that of attraction but differing in its intensity. In the case of the tail, it is a repulsive force. To satisfy these conditions of separation, part in one direction and part in the other, from the comet, we must have an

attraction in the one case exceeding the attraction of gravitation and, in the other, an attraction less than the attraction of gravitation. In other words, these little bodies of hard matter that go off from the comet and follow very nearly in its train are acted on not in proportion to their mass like the force that acts on the planets in their orbits.

I see no escape, myself, from this conclusion. What it means, I must leave to you to decide. Our experiments make it very improbable that the attraction of matter differs in any way from proportion to the mass. It looks to me as though the more natural explanation is that, in some way, the materials which go off from the comet carry with them a load of electricity, or something of that kind, by which they have a permanent repulsion or permanent attraction sufficient to change the orbit altogether, not in kind, but in a steady change, throwing them into a new orbit with a new peroid, and thus scattering them.

What that added force must be, we cannot very well tell, because it differs according to the place in the orbit where the disintegration takes place. If that disintegration takes place near the Sun, it is one thing; if it takes place near Jupiter, it is another. It looks more to me as though there was a disintegration all along the line of the comet's orbit, giving us small particles with all sorts of loads of electricity and all sorts of differences of central attraction and differences of orbits, and thus they get widely scattered so as to give us the showers a long distance from the comet itself. The amount of this change would have to be something like the tenth part, possibly, or something less than that. I should think that all the phenomena could be explained by a change amounting to one-tenth of the attraction; that is, if the small particle carries a load of electricity such as to diminish the attraction to say nine-tenths of the original attractive force of the Sun, or increase it to eleven-tenths, it will explain the phenomena.

If that is the explanation, we come to this further conclusion of interest, that the space through which these comets move is not such that the electricity which the particle carries can be lost. Another practical point would be that, in the discussion of the separation of these comet masses that through the telescope we see going off as the comets pass the Sun, there might fairly be introduced an unknown correction of the force of central attraction.

A MEMBER: Have you gentlemen, who have made a study of this very interesting subject which you have been discoursing on,

arrived at any hypothesis as to what broke up the Biela comet?

PROFESSOR NEWTON: I can only answer as a working hypothesis, in my own mind, is that a mass, not surrounded by an atmosphere, coming down from the cold into a warmer region near the Sun, becomes heated up, and in that heating there is a disintegration going on. If you put the pieces of a meteorite into a vacuum, and heat them, you will get gases that will be something like those which are thrown off from the tail of a comet, and the comet coming down near the Sun, with the hot, scorching effect entirely undiminished by a thick atmosphere, would have pieces broken off, giving fresh surfaces. An immense amount of action of some sort follows, and those pieces would naturally go off under such excitement, carrying with them, as I conceive, a load of electricity. The process goes in on almost all our comets. It is not in Biela alone that we see comets going off to pieces. Scores of comets have shown that same breaking up under the telescope.

LONG FOCUS TELESCOPES.

WILLIAM ROLLINS.

The obstacle to the increase in size and number of large telescopes is the cost. How can this be reduced? As the chief expenses of a great equatorial are the mounting and the dome with its necessary machinery these must be given up. Several other types have been proposed but when telescopes having focal lengths of several hundred feet are used only the horizontal will be practical. At present this is but little used and before its introduction can become general some new form must be given to it to overcome the difficulty of controlling the revolving mirror when the observer is stationed at the other end of the telescope many feet away. To be sure the rate of the driving mechanism can be controlled from any distance by electricity, but for setting the mirror the observer ought to sit beside it.

I propose to reflect the light from the revolving mirror to a fixed mirror distant half the focal length of the objective, and to send it back again to the observer sitting in a chair beside the driving mechanism. In the cut A is the revolving mirror, B the objective, C the fixed mirror and G the eyepiece or sensitive plate. If the focal length of the objective is one thousand feet then the fixed mirror will be five hundred feet from the revolving mirror.

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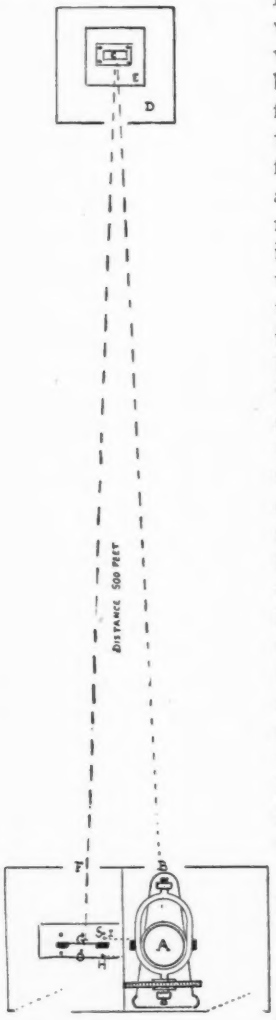

If the reflecting form is chosen there will be no objective and the fixed mirror will be parabolic and placed at its focal length from the eyepiece at G. This form of mounting will be cheap, only two wooden sheds being required, one for the fixed mirror, the other larger and divided by a partition into two rooms, one for the mirror with its driving mechanism, the other, a dark room taking the place of a camera inside which the observer will sit while looking at the image on the plate in front of him thus doing away with a guiding telescope. Rods and wires placed at his side will enable him to control the revolving mirror, while a slide in the partition will admit of direct access to the driving mechanism. A telescope of this type with a focal length of five hundred feet ought to be of value in lunar photography even if the aperture was not larger than twelve inches. The two most probable causes of failure will be the difficulty of building a driving mechanism perfect enough to project a steady image so far and distortion of the image from flexion of the mirrors. I propose to overcome the last by making the mirrors much thicker than is usual.

The following are some of the many points on which I shall be glad of suggestions.

What is the best slow motion? It must be free from lost motion on account of the long beam of light.

Is there any way of making a metallic mirror on glass that will reflect more light and whiter light than silver? Will platinum or iridium answer?

Where shall I get the best seeing, in a tunnel or above ground?

250 MARLBOROUGH STREET, BOSTON.

June 19th, 1892.

Astro-Physics

A STUDY OF NOVA AURIGÆ AND NOVA NORMÆ.*

WILLIAM H. PICKERING.

Considering that there are but about a dozen well authenticated instances of the appearance of new stars in the history of astronomy, it is an interesting fact that the last two should have appeared within two years of one another, and that our earliest record of their existence should in each case be due to photography. This fact taken in connection with the other, that a large proportion of the new stars have appeared within recent times, makes it evident that these phenomena are not of such great rarity in nature as was formerly supposed.

Unfortunately our only record of the spectrum of Nova Normæ consists of a single plate. This was taken in the Bache telescope at Arequipa with small dispersion, the total distance from F to H being but a few millimeters. With such a dispersion it is impossible to determine much more than the general characteristics of the spectrum. A comparison has been made with the spectrum of Nova Aurigæ taken under similar circumstances (see ASTRONOMY AND ASTRO-PHYSICS for January, Plate IV). It is found that each prominent bright line in the Nova Aurigæ has a corresponding bright line in Nova Normæ, and an examination of the negative shows that each bright hydrogen line has an accompanying dark one by its side, in both spectra. In both cases the dark line is toward the more refrangible end of the spectrum. It is impossible to determine the relative velocities of the bodies producing the bright and dark lines from these spectra, but it is evident that they are not very different in the two cases.

Turning now to theoretical considerations, the relative velocity of the components of Nova Aurigæ, although high, cannot be considered as very remarkable, being less than three times as great as is frequently found in that comparatively quiescent body, the Sun. Moreover it is less than twice as great as the velocity which would be produced by a body falling from an infinite distance and just grazing the solar surface. But the high velocity in the case of the Nova had evidently no connection with the mutual attraction of the bodies, since it persisted with but slight diminution for several weeks, the two bodies, according to the generally accepted theory, traversing in the meantime a dis-

* Communicated by the author.

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tance equal to several thousand times the diameter of the Sun. The very fact that so little change of velocity was detected, would indicate that the combined mass of the bodies must have been comparatively small.

The high relative velocity of the components must then have been due either to a very high initial velocity in both bodies, or to an outburst of some sort, which produced a velocity not much higher than we ordinarily find in the Sun. Let us compare now these two hypotheses. The former is the one almost universally adopted, but the latter I think should not be discarded without some further consideration.

On the hypothesis of high initial velocities, it is generally considered that the two bodies are moving nearly in the line of sight, and that Nova Aurigæ for this reason furnished unusually favorable conditions for our investigation of its nature. But apparently Nova Normæ furnished equally favorable conditions, although unfortunately little advantage was taken of them, owing to our not knowing in time of its existence. The probability is about one in one hundred and twenty that in both instances the line of relative motion should be inclined less than thirty degrees to the line of sight, and that the star giving bright lines should in both cases be going the same way. If we merely require that the line of motion should lie within sixty degrees of the line of sight, the probability that the two spectra should be similar is increased to one in eight, but we must admit a maximum actual velocity of the two stars twice as great as that which has been observed by the spectroscope. Moreover, bright line stars are rather rare, and even if we assume that the bright lines were produced by the collision, it is rather singular that in both cases one star only should be so affected. The probabilities, as far as they go, are therefore unfavorable to the correctness of this hypothesis.

Again, how are we to account for the successive reappearances of Nova Aurigæ? Since the relative motion is not due to gravitation, the two bodies must still be receding from one another. It is not probable that each reappearance should be due to a fresh collision with a new and previously unknown body.

It is much easier to attack a hypothesis than to frame a new one, but let us imagine a rather dark Sun, with a viscous surface and gaseous interior. After many centuries of quiescence and contraction a series of eruptive prominences upon an enormous scale burst forth, spreading in every direction, and completely enveloping the star upon all sides. When they first appear, they

present a spectrum of bright lines, but in a few hours the gases first emitted have receded to a considerable distance from the star, and have cooled down owing to the rapid expansion involved by their recession, which causes them to fill a very much larger sphere than that originally occupied by them. Thus the star very soon becomes enveloped in an atmosphere whose outer regions are comparatively cool. The velocity of the emitted gases is so great, however, that they do not fall back at once, as in the case of the Sun, but continue to recede from the star in all directions. In the case of Nova Aurigæ this motion was at the rate of of about 25,000,000 miles a day.

This cold advancing atmosphere produces a series of dark absorption lines. It at the same time cuts off the light from the hot advancing prominences behind it, although a faint reversal of the dark lines in Nova Aurigæ was for a time detected. The hot receding prominences, however, extending away for perhaps millions of miles behind the limb of the star, give out a light whose wave-length cannot be absorbed by the cold advancing atmosphere. They therefore shine with their full brilliancy. The phenomenon may last for several weeks or months until the internal pressure is relieved.

Assuming the maximum total relative velocity indicated by the spectroscope to be 800 miles, the velocity involved in the irruptions upon this hypothesis does not exceed 400 miles per second. A solar irruption is mentioned by Young whose velocity equalled 300 miles per second, and prominences are mentioned by him whose altitude reached 400,000 miles. The reversal of the bright lines in the spectrum of the *Nova* would seem to indicate that the cooled gases were not receding from it indefinitely, but that a downrush of the outer layers had already occurred.

If the bright and dark lines of a *Nova* owe their origin to a series of eruptive prominences in a star, we should expect that the lines would be somewhat irregular in structure, and that these irregularities would vary from night to night. In the case of the dark lines this would be due largely to the existence of bright lines behind them, which they more or less completely concealed. This effect is readily noticeable in the photographic spectra, and is described also by Dr. Vogel (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 901).

If the light of the *Nova* was due chiefly to the presence of prominences, we should expect it to fluctuate appreciably from night to night. If the light was due chiefly to heat generated by collision, it would be more uniform. The *Nova* first appeared, as

shown by the Harvard photographs combined with one of Dr. Wolf's, about December 10, 1891. Ten days later it had doubled in brightness, and it continued to fluctuate, becoming alternately brighter and fainter for the next two months.

If we consider a bright line variable star to be one so far cooled, as to emit very large prominences only at stated periods, as does our Sun for instance, we may consider a *Nova* to be one still further cooled, emitting them perhaps for the last time, but with such intensity that the included gases recede in all directions, forming an atmosphere of extreme tenuity, but whose depth may possibly be measured by hundreds of millions of miles. Such a body, when it had reached a state of equilibrium would resemble planetary nebula in appearance, and probably in its spectrum also.

HARVARD COLLEGE OBSERVATORY,
January 25, 1894.

NOTE ON THE SPECTRA OF COMETS.*

H. KAYSER.

The origin of the bands usually seen in the spectra of comets is not yet sufficiently explained, although numerous researches have been made on this subject. The principal reason for this fact is the want of light in the spectrum which, forcing the observer to use a very wide slit, produces several inconveniences: 1. All fine details of the structure of the bands vanish. 2. The maximum of light is displaced from the apparent edge of the bands towards their middle. 3. The edge is shifted, and 4. Exact measurement of the wave-length is impossible.

By means of photography Campbell (ASTRONOMY AND ASTRO-PHYSICS, Vol. 12, p. 652, 1893) has lately been able to measure wave-lengths with a precision hitherto unattained, and it is therefore interesting to compare his results with those obtained by laboratory observation of carbon bands. Campbell himself has made this comparison between his measurements and those of Professor Runge and myself, but as it seems to me, he has committed some mistakes. Our measurements (*Abhandl. b. Berlin. Acad.*, 1889) refer to the arc, which shows two series of band-groups. The first series contains bands attributed at present, by nearly all spectroscopists, to carbon itself and not to hydro-

* Communicated by the author.

carbon, since Liveing and Dewar have adopted this opinion after having opposed it for a long time. The edges have the following wave-lengths:

First Group.	Second Group.	Third Group.	Fourth Group.	Fifth Group.
(According to Angström and Thalén) 619–595	5635	5165	4737	4382
	5585	5129	4715	4371
	5541	5098(according to Angström & Thalén)	4698	4365
			4685	
			4677(Watts)	

The second series belongs very probably to a compound of C and N, most probably to cyanogen. The edges of these band-groups have the following wave-lengths.

First Group.	Second Group.	Third Group.	Fourth Group.	Fifth Group.
4606	4216	3884	3590	3370
4578	4197	3872	3586	3350
4553	4181	3862	3585	(According to
4532	4168	3855		Liveing and
4515	4158			Dewar.)
4502	4153			

Campbell has doubtless found several groups of these two series. With the eye were observed, the first C group at 601; two edges of the second C group at 563 and 558; three edges of the third C group at 5162, 5129, 509; by photography, five edges of the fourth C group, 4736, 4716, 4697, 4683, 4675. The fifth C group is very much weaker than the others and it is therefore not remarkable that it has been overlooked. Campbell has observed a very bright line at 4366, but this cannot represent the fifth group, as it coincides with the third or weakest edge of the group. He has observed the following of the cyanogen bands; the first three edges of the second group, 4214, 4196, 4178; the first two edges of the third group, 3881, 3870. The fourth and fifth groups are not within the scope of Campbell's observations, but he might have been able to observe the first group. The reason that this has not been the case is perhaps to be found in the fact that this group is much weaker than the other cyanogen groups and that it is covered by the end of the fourth C group which is much stronger and therefore may very easily hide the first Cy group; this is also the case in the arc spectrum.

It can therefore safely be said that the bands of carbon and cyanogen occurring in the arc spectrum have been found in the spectrum of comets. But Campbell has measured still other lines, especially one group situated between 4366 and 4235, which we have not found in the arc spectrum. Campbell supposes that this part of the spectrum has not been covered by our work, but here he is mistaken; there are no strong lines in this part of the

arc spectrum, but only the extremly faint ends of the fifth C group and of the second Cy group. Campbell further speaks of a series of very bright lines between 4098 and 4017 which are also wanting in the arc spectrum. That these two groups of lines or bands belong neither to the carbon bands nor to the cyanogen bands is proved by the regular distribution of the latter, into which they do not fit (Deslandres, C. R. 112, p. 661, 1891). Hasselberg and H. C. Vogel believe that they have found bands of CO in the spectrum of comets; but these unexplained bands do not belong to CO, nor has Campbell found any CO bands. There are, however, some bands in the spectrum of burning hydrocarbon which have often been drawn and measured and have been photographed by H. W. Vogel, and Eder. C. P. Smyth draws, in the spectrum of the acetylene flame (*Edinburgh Astron. Observations*, Vol. 13, 1871), one broad, faint band at 4363 and another strong, narrow one at 4306; Lecoq gives 4368 and 4309, Angström and Thalén for the second 4311, Watts 4313. According to Eder (*Denkschr. d. Mathem.-Naturw. Classe d. Wiener Acad.*, 57, 1890) the first band begins at 4359, the second at 4315. The bands are most distinctly seen on the beautiful photograph of the spectrum of the Bunsen flame by H. W. Vogel (*Sitzungsber. d. Berliner Acad.*, 1888). Campbell gives two of his lines as very bright at 4366 and 4313; I have no doubt that he has here seen the two bands of burning hydro-carbon. It seems worth mentioning in this connection, that the fifth group of the C bands is no more visible in the flame-spectrum photograph than in the spectrum of comets.

Campbell's first group of lines seems thus sufficiently explained, but I do not know of an explanation for the second group between 4098 and 4017. Campbell erroneously compares the first three of these lines with lines measured by us in the second group of Cy, which are much too weak to have been seen in the comet. Some very strong lines appear on Vogel's photograph at 403, which seem to form the edges of a band shaded on the less refrangible side, but it is improbable that they have anything to do with Campbell's lines. This part of the flame spectrum, however, has as yet not been thoroughly examined. Huggins has also observed some maxima of light in the comet *b* 1881 between 410 and 400 (*Chem. News*, 44, p. 183).

The principal result of this comparison is that in the spectrum of comets there are not only visible the bands of carbon and cyanogen which appear alone when vapor of carbon is made luminous by electricity, in the presence of nitrogen, but that some

other bands also appear, the chemical origin of which we do not know, but which it has been proved are seen in the spectrum of burning hydrocarbon. The spectrum of comets, therefore, more resembles that of a *burning* compound of carbon than that of a compound of carbon made incandescent by electricity.

A STUDY OF DIFFRCTION GRATINGS.—FOCAL ANOMALIES.

A. CORNU.*

At the present time diffraction gratings furnish almost the only method for the accurate determination of wave-lengths of light. Although these instruments have been brought to great perfection in the hands of Rutherfurd and Rowland, as regards the definition of the lines in the spectra which they give, they still show occasionally various anomalies which perhaps cast some doubt upon the rigor of the optical principles upon which they are based. It will then be well to study these irregularities in a detailed manner, to determine their laws and causes, and the conditions necessary for estimating their effect upon the accuracy of measurements, to eliminate the errors which they introduce, and hence to attain greater perfection in the construction and use of diffraction gratings.

This somewhat thankless task has frequently claimed my attention since my first observations made a long while ago upon the focal properties of gratings.† I have been led to construct an engine which rules lines which are automatically spaced according to known laws, in such a way that the irregularities whose origin I desired to determine could be produced and increased at will. From the great multitude of practical difficulties which complicate the construction of gratings, I have endeavored to sort out the systematic causes of irregularity, and to separate from them all considerations of a purely geometrical nature. I shall later on have occasion to give a brief description of this engine, whose successive modifications have suggested to me several interesting results, which under different heads I shall ask permission of the Society to communicate successively. Almost all of these are kinematical or geometrical theorems from which has disappeared all trace of the long and laborious methods by which I arrived at them.

* Translated from *Journal de Physique*, September, 1893.

† *Comptes Rendus*, Vol. LXXX, p. 645, 1875; *Association francaise, Congres de Nantes*, p. 376; *Revue Scientifique*, No. 12, Sept. 18, 1875.

Focal Anomalies.

Among the minute irregularities which occur in gratings perfect in definition and in most other respects we must include systematic errors in the position of the focus incompatible with the theory of a perfect grating. All my observations have led me to attribute these anomalies to two distinct and purely geometrical causes:

1st. In the case of *plane gratings*, to the presence of a slight curvature of the ruled surface.

2d. In the case of both *plane and curved* gratings, to the existence of a regular variation in the distance of the rulings.

Very often these two causes co-exist and thus make the law of the grating very complicated.

Anomalous Curvature of the Surface.—This curvature, which as a rule is approximately spherical, and usually convex, is accounted for by the difficulty of obtaining an absolutely plane surface. When the surface is uneven, the definition is imperfect, and the lines are wanting in sharpness. Whenever the ruled surface is of the nature of a surface of the second degree, having a plane of symmetry parallel to the rulings, the lines in the spectrum may be perfectly well defined. Even the inevitable astigmatism may be corrected by a method which I have indicated elsewhere.*

Hence in the present study of the focal properties of gratings, we may set aside the case of curvature of the surface in a plane parallel to the rulings, and consider only the curvature normal to them. This amounts to supposing the grating ruled on a cylindrical surface, of which the rulings are generating lines.

The ruled surface is then characterized simply by its radius of curvature, R, so that all problems connected with it, being reduced to cases of plane geometry, are much simplified.

Anomaly in the Distribution of Rulings. Characteristic Law.—Continuous inequality of spacing of the lines is explained by the difficulty of ruling them at exactly equidistant intervals. We may represent the distance, s, between the lines, beginning with a line which passes through the origin, by the formula†

$$s = bt + c^2t,$$

in which the variable t (representing for instance the number of turns or fractions of a turn of the screw of the dividing engine), may take the value 1, 2, 3, —, n. The disturbing term ct^2 is pos-

* *Ann. de Chim. et de Phys.*—6th Series, Vol. 7, p. 19.

† A term of small value in t^3 would introduce sensible aberrations into the formation of focal images. The gratings which we consider, however, are too nearly perfect for these to be appreciable.

itive, ($c > o$), when the width of the spaces increases with t; and negative, ($c < o$), in the contrary case.

Kinematical Interpretation of the Above Law. Characteristic Parameter.—This law of the increase of spacing of the lines may be interpreted by a diagram which takes into account the relation between the coefficients b and c.

Suppose the grating to have been ruled by means of a screw, turning with equal increments of angle δt, t increasing positively. If we have $c = 0$ the rulings will be equidistant, the step of the screw being constant, and the screw-thread will form a perfect helix, whose development upon a plane surface is a straight line. If $c > 0$ the rulings become more and more widely spaced, and for $c < 0$ they become more and more closely packed; the screw has then a variable step, which if the screw is long enough will end, in one direction or the other, according to the sign of c, by becoming zero when

$$\frac{ds}{dt} = 0.$$

This will take place at the distance given by

$$s_0 = -\frac{b^2}{4c},$$

which we shall designate later on by

$$-\frac{1}{2}\,\mathrm{P}.$$

Hence we easily conclude:—

When a grating shows a progressive variation in the spacing of its lines represented by the formula $s = bt + ct^2$, we may consider it as having been ruled by means of a screw, the development of whose thread upon a plane surface would be an arc of a parabola,* of which the axis is parallel to that of the screw. The distance of the apex of the parabola from the origin,

$$s_0 = -\frac{b^2}{4c},$$

is a characteristic parameter of the screw, and of all gratings ruled by means of it, for it is independent of the number of subdivisions of a revolution, that is, of the mean distance between the lines.

The Laws of Focal Anomalies.—We shall now prove the following important result:—

* The *parabolic helix* is made use of in the rifling of gun barrels.

The focal anomalies of the grating in the plane normal to the rulings are completely defined by two linear constants, the radius of curvature R, of the surface, and the parameter P, of the screw of the ruling engine. These two constants can be derived from optical and geometrical data of experiment by two very simple equations which may be found as follows:—

Let ρ, ρ' (Fig. 1) be the respective distances from M, the center of the grating, of the source A, and focus A′ of the incident and diffracted pencils.

α, α' the angles which the axes of these pencils make with the normal at the point of incidence.

R, the radius of curvature of the section MS of the grating perpendicular to the rulings.

P, the characteristic parameter of the law of distribution of the rulings.

ε, the mean grating-space.

Consider a cylindrical wave emanating from a point A and meeting two consecutive lines on the grating at M and M′. The difference of path of the two rays will be

$$AM - AM', \quad \text{or} \quad \rho - (\rho + \delta\rho) = -\delta\rho.$$

We have also

$$-\delta\rho = \delta s \sin\alpha$$

$$\text{and} \quad \rho\delta\varepsilon = \delta s \cos\alpha$$

in which δs is the small grating-interval MM′ corresponding to the variation δt in the equation $s = bt + ct^2$, (where s is reckoned positive in the direction MS), and α is the angle CMA. Each of the rulings through M and M′ becomes a center of diffracted waves, and two such waves will reach any point A′ in the same phase, if the difference of path M′ P + M′ P′ is a positive or negative integral number of wave-lengths. In that case we have

$$\delta\rho + \delta\rho' = -m\lambda \quad \text{or} \quad \delta s\,(\sin\alpha + \sin\alpha') = m\lambda, \qquad (2)$$

a wave-length, as well as a grating-space, being considered infinitesimal.

If we consider a third line M″, (defined by a new constant increment δt of the variable t) as associated with the second line M′, the condition that the diffracted ray from M″ shall reach A′ in the same phase, is the same as before, but t, α and α' must be replaced by $t + \delta t$, $\alpha + \delta\alpha$, and $\alpha' + \delta\alpha'$. $m\lambda$ and δt remain constant. Hence, differentiating equation (2)

$$\delta^2 s\,(\sin\alpha + \sin\alpha') + \delta s\,(\cos\alpha\delta\alpha + \cos\alpha'\,\delta\alpha') = 0. \quad (3)$$

Denoting the infinitesimal angles, C, A and A′, by $\delta\omega$, $\delta\varepsilon$ and $\delta\varepsilon'$ we have

$$\left.\begin{aligned} \delta\alpha &= \delta\omega - \delta\varepsilon \\ \delta\alpha' &= \delta\omega - \delta\varepsilon' \end{aligned}\right\} \quad \delta s = \mathrm{R}\,\delta\omega \quad \left\{\begin{aligned} \rho\,\delta\varepsilon &= \delta s\cos\alpha \\ \rho'\,\delta\varepsilon' &= \delta s\cos\alpha' \end{aligned}\right. \quad (4)$$

Equation (3) may now be written, on substituting for $\delta\alpha$ and $\delta\alpha'$, in terms of δs, and dividing by δt^2, in the form

$$\frac{\delta^2 s}{\delta t^2}(\sin\alpha + \sin\alpha') + \left(\frac{\delta s}{\delta t}\right)^2\left(\frac{\cos^2\alpha}{\rho} + \frac{\cos^2\alpha'}{\rho'} - \frac{\cos\alpha + \cos\alpha'}{\mathrm{R}}\right) = 0 \quad (5)$$

The quotients $\frac{\delta^2 s}{\delta t^2}$ and $\frac{\delta s}{\delta t}$ may be replaced by the derivations $\frac{d^2 s}{dt^2}$ and $\frac{ds}{dt}$. At the *middle-line* of the grating, which passes through the origin, and for which therefore $t = 0$, we have from the equation $s = bt + ct^2$,

$$\frac{d^2 s}{dt^2} = 2c, \qquad \left(\frac{ds}{dt}\right)^2 = b^2.$$

The quotient of these is $\mathrm{P} = \frac{b^2}{2c}$.

Equation (5) now takes the symmetrical form

$$\frac{\cos^2\alpha}{\rho} + \frac{\cos^2\alpha'}{\rho'} = \frac{\cos\alpha + \cos\alpha'}{\mathrm{R}} - \frac{\sin\alpha + \sin\alpha'}{\mathrm{P}} \quad (6)$$

Also since $e = b\delta t$, equation (2) may be written

$$e\,(\sin\alpha + \sin\alpha') = m\lambda,$$

e representing the *mean grating space*. Such are the laws of focal anomalies.

Discussion of these Formulæ. Conjugate Focal Curves.—Equation (6) gives the relation connecting the focal distance $\rho' = \mathrm{MA}'$ (Fig. 1) of a cylindrical wave of wave-length λ, diffracted in the spectrum of the mth order, with the distance of the source, $\rho = \mathrm{MA}$.

1. This equation being symmetrical with respect to ρ and α on the one hand, and ρ' and α' on the other, the points A and A′ are true conjugate foci; hence we may either regard A as source and A′ as focus, or the reverse.

2. For any position of the source, (ρ = const., α = const.), equation (6) leaves the position of the focus A′ indeterminate,

hence this equation represents the geometrical focus in polar coordinates (ρ', α') of all positions which the focus of the diffracted pencil conjugate with the source can occupy in the plane of diffraction. This locus is thus the *focal curve* corresponding to the given position of the source.

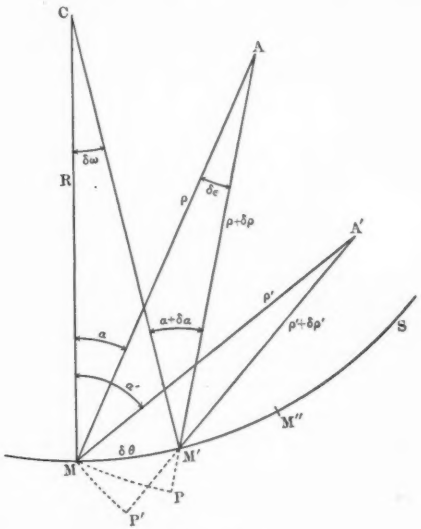

FIGURE 1.

3. The *focal curve* A′ does not in general pass through the source A; hence there is a *family of focal curves*, whose parameter is defined by the substitution of the coördinates of the source, (ρ, α) in equation (6).

4. The equation of the locus of positions of the source A which correspond to the same focal curve A′ is clearly

$$\frac{\cos^2\alpha}{\rho} - \frac{\cos\alpha}{R} + \frac{\sin\alpha}{P} = k \qquad (8)$$

The equation of the focal curve A′ must in this case be

$$\frac{\cos^2\alpha'}{\rho'} - \frac{\cos\alpha'}{R} + \frac{\sin\alpha'}{P} = -k \qquad (8a)$$

in order to satisfy equation (6). It differs from the preceding only in the sign of the constant k.

These two families of curves are therefore *conjugate*.

Principal Focal Curve.—The parameter k may have the value

zero, in which case the two conjugate curves coincide, and their common equation is

$$\frac{\cos^2 \alpha}{\rho} - \frac{\cos \alpha}{R} + \frac{\sin \alpha}{P} = 0. \qquad (9)$$

This curve has thus the property of passing through all the foci and the source. It is *unique* for a given grating, and depends only upon the radius of curvature R and the parameter P. It is clearly independent of the mean grating-space.

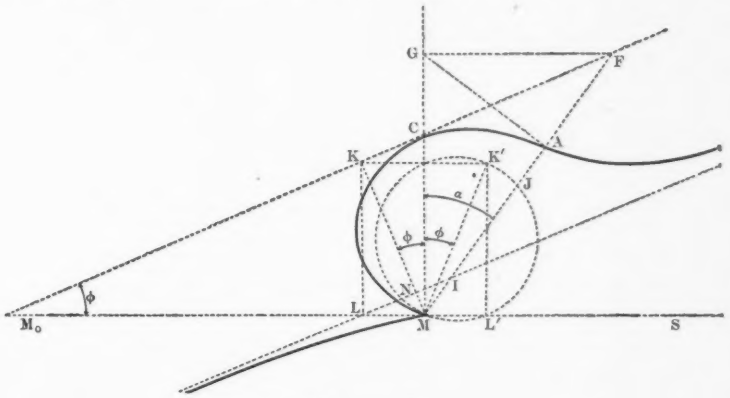

FIGURE 2.

I propose to call this the *principal focal curve*.

This curve takes various forms depending on the ratio between R and P. These forms are derivable from the type of the Cissoid of Diocles, and return into the type itself when $R = \infty$. Equation (9) may be put into the following forms:

$$\rho = \frac{\cos^2 \alpha}{\frac{\cos \alpha}{R} - \frac{\sin \alpha}{P}} = \frac{PR}{H} \frac{\cos^2 \alpha}{\cos (\alpha - \varphi)} \qquad (10)$$

where

$$R = H \sin \varphi \qquad \text{and hence } \tan \varphi = \frac{R}{P}$$

$$P = H \cos \varphi \qquad H^2 = P^2 + R^2$$

which leads to a simple geometrical construction (Fig. 2).

The above equation may be written in the form

$$\rho = R \cos \varphi \left(\frac{\sin^2 \varphi}{\cos (\alpha + \varphi)} + \cos (\alpha - \varphi) \right) \qquad (11)$$

which shows that the radius vector ρ is, like that of the cissoid, the sum of two others, namely of a straight line and of a circle. This leads to a second method of construction.

Any point A may be found from equation (10), by the aid of the straight line M_0C which joins the center of curvature C of the grating, and the point M_0, taken such that $MM_0 = P$. Let fall a perpendicular FG upon MG from the extremity of a radius vector of this line, and from G, a perpendicular GA upon the radius vector. A is then a point on the curve. The curve has as an asymptote the line LN parallel to MF when $\alpha = 90° - \varphi$, and distant from the origin by an amount $MN = R \cos \varphi \sin \varphi$, as may be seen by constructing the perpendiculars MK upon M_0C, KL upon M_0M and LN upon MK.

The second method of construction, derived from equation (11), consists of prolonging the radius vector MJ of the circle described on MK′ as diameter, by an amount equal to MI, the radius vector of the asymptotic line LN defined previously. The diameter of this circle is $MK' = R \cos \varphi$, and its equation is

$$\rho = R \cos \varphi \cos (\alpha - \varphi).$$

The point K′ is taken symmetrical with K about the line MC.

The figure corresponds to the case $c > 0$, $P > 0$. The rulings will then be more widely spaced toward the right, for $P = -2s_0$. (*Vide supra*).

This second definition of the principal focal curve leads to several immediate verifications by considering known results as special cases of the above.

Suppose the grating to become more and more nearly perfect as regards spacing, while the curvature remains unchanged. The point C will remain fixed while M_0 approaches infinity. In the limit the focal curve reduces to a circle [upon the diameter MC], and this is the circle employed by Rowland in his admirable concave gratings.

If in the concave grating there is still a minute systematic error of ruling, the angle φ is not absolutely zero, and the principal focal curve is sensibly a circle whose diameter through M makes the very small angle φ with the normal to the grating. This is the result recently obtained empirically by Rydberg (*Academie de Stockholm, t.* XVIII., No. 9).*

Finally, in the reverse case, in which the grating is sensibly plane, while the spacing shows a marked systematic variation, the point C is at infinity and φ is a right angle. The principal

* [Also, *Phil. Mag.*, March, 1893].

focal curve is then a cissoid, having an asymptote which passes through M_0 and is normal to the grating surface. We thus find the arrangement of the foci of the spectra which I pointed out in my first researches.

I confine myself at present to these purely geometrical results, reserving for a future article the description of experimental methods which furnish numerical verifications of these laws.

IS THERE OXYGEN IN THE ATMOSPHERE OF THE SUN?*

N. C. DUNER.

This question has been much discussed; its solution presents difficulties arising, perhaps, from the inadequacy of the methods which have been employed for studying the spectra of gases.

The ordinary method consists in enclosing the gas in tubes, and causing it to be traversed by an induction spark, with or without a condenser. There is, however, no proof that the spectra thus obtained are identical with those which are seen in examining a celestial body whose atmosphere contains the same gases under different conditions of pressure and incandescence.

It is true that the spectrum of hydrogen obtained with spectrum tubes is identical with that given by the chromosphere; but it is known, on the one hand, that there is another spectrum of hydrogen which has not been found in any celestial body, and on the other hand, that hydrogen differs essentially from other gases, such as oxygen, nitrogen and chlorine, and is to be regarded rather as a gaseous metal than as a metalloid. It is known, moreover, that with the exception of carbon, no certain spectroscopic proof has been found of the presence of a single metalloid in the stars, or in comets, or in nebulæ. But is it possible to believe that all the celestial bodies, including the Sun, are deprived of all metalloids, while oxygen, in the form of water vapor, has been proved to exist in the atmospheres of several of the planets?

Let us now pass to a consideration of the spectral characteristics of oxygen. There are four different spectra of emission which are believed to belong to oxygen, namely; two line spectra, one band spectrum, and a continuous spectrum. In the spectrum of the Sun neither the line nor the band spectrum has been seen, and as for the continuous spectrum, it would evidently be impossible to prove that it exists. M. Egoroff has demonstrated that in ad-

* Translated from the *Comptes Rendus*, cxvii, No. 26, Dec. 26, 1893.

dition to these there is an absorption spectrum of oxygen, and that it is represented in the solar spectrum by the two strong bands A and B. M. Cornu having subsequently proved that, setting aside some relatively faint lines produced by metals in the Sun's atmosphere, there is a complete identity between the band α and the bands B and A, the conclusion may be drawn that this band is in all probability also a band of oxygen.

Sir D. Brewster has already proved that all these bands are *telluric bands*, that is to say, that the gases which produce them are found, at least in a large measure, in the atmosphere of the Earth. But while it is easy to prove that the bands due to water vapor are exclusively telluric, since they entirely disappear during excessively cold weather, it is otherwise in the case of the bands of oxygen. The latter are no doubt weaker when the Sun is high, or when they are observed at stations greatly elevated above the Earth's surface, but still they are always visible, at least in their stronger parts. It may consequently be supposed, and in fact it has been supposed, that oxygen in the solar atmosphere may have contributed to their formation. I believe, however, that I can demonstrate that these bands are purely telluric.

Let us suppose that the bands A, B and α are partly telluric and partly solar. There would then be a particular place on the surface of the Sun,—namely, that place which is neither approaching nor receding from the point of observation by the combined action of the rotary and orbital motions of the Earth and Sun,—where the two parts, solar and terrestrial, of a line belonging to one of the bands, would have the same wave-length. At other parts of the surface, however, the wave-length of the solar part would either exceed or fall short of that of the telluric part, and it is easy to see that in the neighborhood of the solar equator the separation of the parts would become visible in a spectroscope of sufficient power, so that the line would appear double, having two components differing more or less in intensity.

Now in my researches on the rotation of the Sun, I have examined the lines in the band α hundreds of times. At points on the east limb in the vicinity of the solar equator, the wave-length of a given line differs from that of the same line on the opposite limb by 0.12 revolutions of the micrometer of my spectroscope,—a quantity large enough to sensibly alter the configuration of the small group of lines to which my observations were ordinarily directed. Consequently if the lines had not been exclusively terrestrial, but partly of solar origin, the two different parts would have appeared separated by so large a space that the duplicity

of the line could not have escaped even an inattentive observer, and even if the intensity of the components were quite different.

There is another series of observations which affords an ample confirmation of my own, namely, the researches of M. Cornu on the groups A, B and α. In these observations M. Cornu recognized the different nature of the lines belonging to these groups by the *oscillations* of the solar rays when the slit of the spectroscope was made to pass rapidly across the image of the Sun. In so oscillating, the oxygen lines should have appeared double if they have a double origin, solar and terrestrial, and this appearance could not have escaped the practiced eye of M. Cornu. On the whole, I do not hesitate to maintain that *the bands* A, B *and* α *have a purely telluric origin.*

Is there then no oxygen in the Sun? I have already, in what precedes, expressed my opinion on this subject. But how is it that none of the spectral characteristics of oxygen are to be seen? The explanation is not easy, and different answers have been given. It has been supposed that the metalloids are dissociated by the extremely elevated temperature of the Sun, which is explaining an incomprehensible phenomenon by a hypothesis scarcely more intelligible. The fact can be invoked, as it was by M. Scheiner, that in spectrum tubes the incandescence of metalloids ceases in the presence of metallic vapors; but I have already observed that one can hardly be certain that the phenomena observed in spectrum tubes are applicable to the celestial bodies; moreover, in the first case the incandescence is produced by the electric spark, while in the second it is the result of heat only. It may well be supposed, when we bear in mind the diversity of the spectra of the metalloids, that in the particular conditions of the solar atmosphere the spectrum of oxygen is of such a nature as to preclude detection. And finally we may suppose, with M. Egon de Oppolzer, what is perhaps not the least reasonable condition, that in the absorbing layers of the Sun, oxygen exists in such a state of rarefaction that it is incapable of producing any sensible absorption.

But these are merely so many purely hypothetical speculations, which it is useless to dwell upon here.

A SHORT REVIEW OF MY THEORY OF THE SUN.*

A. BRESTER JR.

§ 1.

EXPLANATION OF THE AUTOMATIC CHECK WHICH PREVENTS ANY CONSIDERABLE MOTION IN THE INTERIOR OF THE SUN'S MASS.

The interior tranquility of the Sun is not to be thought of merely as the "conditio sine qua non" of its permanent stratification, but as *a priori* much more probable than the ordinary theory of violent agitation. Every one knows that the single cause of terrestrial cyclones lies in the disturbing action of the Sun. Since an analogous cause does not exist on the exterior of the Sun its atmosphere will there be just as calm as would ours, if the Sun did not exist. "But," someone will ask "is it not the unequal cooling that causes these supposed disturbances?" To this I reply that the incandescent and dissociated gaseous state of the Sun's mass makes all unequal or sudden cooling impossible.

The solar gas in fact is so hot that it must contain very many separated molecules always ready to reunite whenever, by continual loss of heat, their temperature is sufficiently lowered. In this manner, they hinder, by the heat produced in their union, any appreciable fall in temperature. Just as steam, in losing heat, cannot be cooled down below 100° until all its mass is converted into water, so the incandescent solar vapor cannot be lowered in temperature so long as any of its material capable of being condensed at that temperature is not yet so condensed.

Now if the incandescent solar gas has not already reached this final state, all cooling there is impossible; and *a fortiori*, all unequal cooling. A quiet interior should, therefore, not surprise us. The molecules always ready for combination at the least loss of heat come under two heads:

(1.) There are those vapors cooled down to their dew-point which are found throughout the solar atmosphere and in the photospheric layers, and bear visible witness to their saturation by the fog of condensed particles with which they fill more or less the exterior layers of the Sun. In a mist uniformly saturated the temperature will always remain stationary, however great the quantity of heat there absorbed or developed.

* Read at the Congress of Astronomy and Astro-Physics, Chicago, August, 1893. Continued from December number.

(2.) There are also molecules of dissociated matter cooled to a point where their re-combination becomes possible. These molecules coöperate with those of saturated vapors to maintain a constant temperature; because, in continually combining in proportion to the loss of heat through radiation, the new heat thus produced will counteract the cooling, and will not, on the other hand, produce any increase of temperature, since such an increase would be immediately absorbed, not only by the evaporation of the mist already mentioned, but also by the chemical dissociation of those same molecules which in combining were the source of the new heat.

If, in general, solar phenomena teach us that the Sun is in a state of repose and that this quiet is such that the Sun, in spite of its gaseous state, presents the appearance of a solid, made up of superposed layers, which do not vary, and of parallel zones, always the same; and if here periodic phenomena are regularly repeated; then, I say, this quietude agrees very well with the known properties of matter. Because we see in the physical and chemical condensations by means of which the Sun renews its lost heat without cessation, an automatic check hindering those motions which, without it, cooling would certainly produce.

§ 2.

Explanation of the Intermittent Outbreaks of Heat Which this Check is to Prevent.

If all the vaporized and dissociated molecules required but an infinitesimal lowering of temperature to condense or to combine all at once, there would be perfect tranquility as a result; but none of the characteristic phenomena now observed would follow therefrom. Among all these segregated molecules, there would necessarily be some, which, not being ready to combine at the least reduction in temperature, would unite in an intermittent fashion and would, in this manner, cause the periodic production of *vis viva* which I have named "heat eruptions," (*eruptions de chaleur*). Such would be the separated atoms, A and B, whose association into the molecule AB is not hindered by too high a temperature but by too great a number of other intervening molecules. It is clear, of course, that the innumerable molecules, R, interposed between the molecules, A and B, would act as an obstacle to their union. Thus it was, for instance, that in cooling a mixture, A + B + R, of dissociated water, A + B, and of carbonic

acid gas, R, Deville* could collect an explosive gas, that is, of molecules, A + B, which, according to other experiments of Deville, would surely have been recombined had they not been prevented by the presence of the molecules, R, of carbonic acid. If, therefore, in some solar stratum, similar combinations, A + B + C, are being produced by a constant loss of heat, the molecules, A + B, although ready for association, can not immediately combine. The molecules R, would then operate to oppose this by a state of aggregation corresponding to the temperature of the stratum in question. Now as such a radiation would diminish very rapidly, the number of the molecules R (especially if their state of aggregation is that of a mist-like condensation of a saturated vapor), the remaining molecules, R, would finally become too few in number to hinder the union of A + B. Then all at once and so far as possible these molecules would unite. They would produce a sort of *heat eruption*. But the heat then produced, while opposing further cooling, could never cause, no matter how great it might be, the least rise in temperature, since the heat would be at once absorbed by the vaporization or the dissociation which it will again cause in the molecules, R, recently condensed. And this work ended, the temperature would never be higher than before, since an increase of heat would cause an increase of dissociation, while, the only source of heat is the formation of new molecules AB.

At the termination of this outburst of heat we find again the molecules R, which that heat had separated, in the same state as at first—that is—in the state in which by their presence, they prevented the combination of the remaining molecules A + B; and, consequently, by their condensation preserved the solar heat and in this manner, made way for a new outburst again to disunite them; and so on.

It is clear, moreover, that a heat eruption will never be produced in all points of the same spheroidal layer at the same time. For at different points of the stratum there would be

(1) Some slight inequality in chemical composition and

(2) A much more important inequality in the emissive and reflective power of neighboring layers more or less misty in their composition—an inequality which will temporarily cause a loss of heat peculiar to each part of the layer. Now as it is only this loss which, in connection with the chemical composition, determines

* E. H. Sainte-Claire Deville; *Lecons sur la dissociation propessees devant la Societe chimique*, 1864, Debray; *Dict. de chimie par* A. Wurtz. Art. *Dissociation*, p. 1174.

an *eruption de chaleur*, each outburst will, in consequence, have an extent, a duration and a motion entirely its own.

Such is the explanation of the manner in which the chemical energy of the dissociated elements can oppose, in an intermittent manner, the cooling of the Sun, and of how it can produce in the exterior layers, periodic heat-eruptions which are repeated at sensibly equal periods, while the recombination constantly going on in the dissociated strata prevents any obvious change in their chemical composition.*

These heat-eruptions which, while maintaining the temperature constant, prevent all interior movement in the solar mass, would take place perhaps unperceived if they were not characterized by sharply defined luminous phenomena which are shown to be exactly such as are required by theory. It is clear that where a heat eruption is forming there will most often be cloud-like or misty matter, R, which, being highly incandescent, will emit a white light with a continuous spectrum, and that where such an eruption is already formed, on the contrary, that white light preexisting will disappear again. But the disappearance of that white light will not be the only luminous effect of a heat eruption; for that eruption being caused by a chemical combination of the gaseous molecules A and B, will produce a spectrum of bright lines, that is, a "chemical luminescence" like that which one observes in our laboratory flames, where, according to recent work of a large number of physicists,† it is independent of the temperature, the principal, if not the only cause of bright line spectra. Such are the intermittent luminous phenomena which my theory of a gaseous, tranquil globe with periodic heat eruptions leads us to expect, and such are also the intermittent luminous phenomena which the Sun actually presents.‡

* We have in these outbursts of heat which occur in the Sun (1) *the condensation of vaporized matter*, and (2) *the chemical combination of dissociated matter*. These two act as intermittent sources of heat, each, so to speak, repairing the damage which the other has done. But there is in the Sun still another source of heat *viz.* (3) *the contraction of the mass of the Sun*. Possibly, in my theory, sufficient account has not yet been taken of this third source, for, as we shall see later, it exerts great influence upon the intermittent character of the *eruptions de chaleur*.

† E. Wiedemann: *Pogg. Ann.* 37, pp. 177-248; R. v. Helmholtz: *Die Licht und Warme strahlung verbrennender Gase*. Berlin, 1890; W. H. Julius: *Die Licht und Warmestrahlung verbrannter Gase*. Berlin, 1890; Langley and Very: *Amer. Jour. Sci.* 1890, Vol. 40, pp. 97-113; G. Wiedemann: *Lehre von Elec.* IV, p. 526; W. Siemens: *Wied. Ann.* 18, p. 311; E. Pringsheim: *Wied. Ann.* 45 (1892), p. 428, *Nature*, June 8th, 1893.

‡ The manner in which solar phenomena accord with the predictions of my theory is all the more remarkable since in developing my theory I never had the Sun especially in view, but rather, the explanation of *variable red stars*. This explanation was published in 1888 and may be briefly summed up as follows:

§ 3.

Explanation of the Peculiar Rotation of the Photospheric Layer and of some Local Disturbances which that Rotation should Produce.

Before studying the intermittent luminous phenomena which the Sun exhibits, we must first say a word or two concerning its rotation, and the configuration of its gaseous portion.

The cloudy photospheric layer of the Sun shows no sensible flattening, the solar gas itself nevertheless has an appreciable equatorial enlargement which has often been observed and photographically established.* The independence of these two configurations has, however, but little to surprise us. For the matter which gives to the photosphere its dazzling whiteness, not being gaseous and having, in consequence, a specific gravity in general quite different from that of the gaseous stratum in which its condensation took place, would naturally be repelled from that layer towards new ones, where, preserving its initial speed and changing its angular velocity, in consequence it would come to rest at a surface where various circumstances (especially the specific gravity of the medium), had given it a stable position in the photosphere.

The independence of the photosphere, as regards the stratified and flattened gaseous layers, being explained in this way, it is clear that the photosphere, receiving at various latitudes the condensed matter from upper strata, will in consequence rotate the more quickly the nearer one approaches the equator. It is clear, I say, that the photosphere should have at different latitudes not only different angular velocities, diminishing toward the poles, but also a chemical composition which (in the photosphere as well as in the atmosphere which covers it) will be the same only in two parallels equidistant from the equator.†

In variable red stars, the intermittent condensation of molecules R, produces obscuring clouds, at intervals, upon the exterior of the star.

The thicker these clouds become, the more they shut off the light from the interior of the star, thus preparing the way for a new maximum. For as soon as the star reaches its minimum, the condensation of the molecules R will have attained a certain limit. The molecules A and B will now begin to reunite, thus producing an "*eruption de chaleur*," which, in turn, will vaporize the clouds of the minimum, and will restore the maximum by opening to view the constantly brilliant central portion of the star.

* Secchi: *le Soleil*, I, p. 340, 390; II, p. 482, etc.; Young: *The Sun*, p. 173; Lockyer: *Chem. of Sun*, p. 424; *Solar Physics*, p. 49; Holden: *Himmel und Erde*, I, p. 443.

† If this explanation of the equatorial acceleration is correct, the coronal atmosphere should take part in the rotation of the Sun. This prediction has been confirmed by the observations of Deslandres during the solar eclipse of 16th of April, 1893. In two equatorial regions of the corona, 180° apart, he found the relative velocities in the line of sight to be from 5 to 7 kilometers per second.

Deslandres: *Compt. Rend.*, May 15, 1893, and *Nature*, May 25, 1893.

The main part of the gaseous mass of the Sun has not only a shape different from that of the photosphere, but it rotates at a different rate. *For while the main part of the Sun's mass turns on its axis as a whole, it is only in the cloud-like photosphere that there exist those variable angular velocities discovered by Carrington.* Here is a prediction which the observations of Mr. Crew have clearly confirmed. He has proved by means of the displacement of the Fraunhofer lines that the rate of rotation of the absorbent solar gas is the same in all latitudes, the period being approximately 26.23 days. Wilsing has found likewise that the faculæ rotates with uniform angular velocity at all latitudes. Since the uniformity of that value seems to prove that it is only at the photospheric level that there exist these anomalous angular velocities, it is remarkable that the velocity determined by Wilsing is greater than that obtained by Crew. Very recently, however, in repeating the researches of Wilsing, Belopolsky has found a smaller value exactly equal to that of Crew.*

Granting that the main part of the Sun rotates as a solid in about 26 days, the question now comes up of determining the different angular velocities in the different zones of the photosphere. These different velocities are not to be identified with those of spots, for all astronomers agree that, from time to time during the whole life of a spot, and especially when it is changing its form, it makes, now and then, those sharp advances, leaps, as it were,† and these incidental accelerations are not due to a temporary increase of velocity of the photosphere, which carries them with it. For a group of spots never shows a common acceleration, but each preserves its individuality.‡ If we can grant that as long as the spots do not change form, they have the same velocity as the photospheric clouds, then, whenever they do change form, they rotate more rapidly than the photosphere. And as the law of Carrington and Spoerer is deduced only from the *mean* motion of spots, *the velocity of spots, such as results from that law, is greater than that of the photospheric clouds.*

If, consequently, at a latitude of 21°, the spots rotate in 26 days just as the main part of the gaseous mass of the Sun, the photospheric clouds will there rotate less rapidly, and, in order

* H. Crew: *Amer. Jour Sci.*, Sept., 1889; *ibid*, Vol. 35, p. 159; Wilsing: *Publ. Astrophysik Obser. Potsdam.* 1888. *Astr. Nach.* No. 3153; Belopolsky: *Astr. Nach.* No. 3158. According to Wilsing the result of Belopolsky is 0°.56 smaller that he himself gave it. Introducing this correction, one obtains for the daily rotation 13°.71, which is practically identical with value obtained by Crew.

† Young: *The Sun*, p. 110; Secchi: *le Soleil*, I, p. 141.

‡ Spoerer: *Sounen flecken beob. im J. 1880-84*, p. 422.

to see these clouds rotating with the velocity of the body of the Sun, they must be observed at a lower latitude.

My theory requires that this lower latitude lies between the parallels of 10° and 15°, that is, in the zone where the extended observations of Carrington and Spörer* have shown that spots are produced most abundantly. Now this is a requirement which I think in accord with the detailed observations of Dunér.

Dunér sought, as did Crew, to determine the angular velocity of the absorbing layer of the Sun by means of the displacement of the Fraunhofer lines. But he obtained quite another result. For, in place of finding an angular velocity everywhere the same, he discovered on the contrary, that the velocity diminished like that of the spots, as the latitude increased.† At first sight, the observations of Dunér and Crew seem to contradict each other, but, upon a closer examination, it is not difficult to explain their apparent contradiction. "All the Fraunhofer lines,"—to quote Young,‡ "are not due entirely or even principally to the gaseous layer situated above the upper level of the photosphere. . . . The principal absorption probably takes place in the interstices between the photospheric clouds, and below the general level of their upper limit." Now it is evident that in the interstices of these clouds, the absorbing gas will have acquired the velocity of the clouds themselves and consequently, the Fraunhofer lines produced by it will indicate velocities diminishing with latitude. *Only those lines which originate above the general level of the photosphere can show the real motion of the solar atmosphere.* Now those were just the lines which Crew measured. For no less than seven of them are found in the catalogue of 273 lines which Young on Mt. Sherman saw reversed on the exterior of the photosphere; while this catalogue does not contain either of the two lines 6301.72 and 6302.72 which were the only ones used by Dunér.

There is then no difficulty in admitting that the velocities obtained by Dunêr are not those of the solar atmosphere, properly so-called, but those of the photospheric layer. Their numerical values correspond completely with the requirements of my theory. For they show (1), that the photospheric strata everywhere rotate more slowly than the spots, and (2), that at the lat-

* Spoerer: *loc. cit.* p. 198; Carrington: *Redhill Obs.*; Secchi: *le Soleil*, I, p. 136, Tab. B; Young: *Elements of Astronomy*, p. 129. One must not confound the latitude of 11°, where spots appear most frequently with the mean latitude of spots at maximum, *viz.*, about 16°.

† Duner: *Astr. Nach.*, No. 2968, p. 270. *Sur la rotation du Soleil.*

‡ Young: *The Sun*, p. 63; Scheiner: *Spectral analyse der Gestirne*, p. 194, 196, etc.

itude of 11° is found the only parallel where the angular velocity of the photospheric clouds is exactly equal to that of the atmosphere which covers them. At higher latitudes, the photospheric clouds rotate more slowly than the upper atmosphere; at lower latitudes they rotate faster.

As this difference in velocity between the photospheric clouds and the atmosphere covering them should cause in the superficial layers of the clouds either an acceleration or retardation, it would be only at the eleventh parallel where the superficial layers would not be seen slipping over the deeper layers like a veil.

In all other parallels these veils will be formed, and they will slip the faster the more they recede from the eleventh parallel towards the poles or the equator. Consequently, if the spots are holes in the photosphere, these holes will present openings which are larger, the nearer they are to the eleventh parallel. At latitudes quite different from 11° their orifices will be rapidly closed as it were by a sliding cover. And at latitudes of 35° or more this screen will be already there even before the spots have had time to develop. I have in fact calculated that while this screen at 15° requires not less than 19 days to traverse the length of a large spot (measuring one geocentric minute), the screen at 30° will cover such a spot in only four days. At 45°, the time required is only about two days. Now even in the "*zones royales*" (where my theory demands still other causes better adapted to the explanation of spots), any considerable spot requires several days or weeks* for development, while any spot occurring at higher latitudes will clearly remain invisible, or at most, will appear as one of those "veiled spots," those "*crateres sans tache*," or "rudiments of spots" which Secchi and Trouvelot have observed all over the surface of the Sun.†

From the preceding considerations, one is led to believe that, no matter how great may be the interior tranquility of the Sun, the anomalous rotation of the photospheric zones produces there nevertheless some agitation. But as that agitation (insensible at the two eleventh parallels) takes places without possible change of temperature, it remains localized in the photospheric level where it is produced, and can disturb the solar gas only in the immediate neighborhood of the photospheric layer.

Let us now see how in our tranquil Sun there are heat eruptions which hollow out the photospheric spots and there kindle the prominences and the coronal rays.

* Young: *The Sun*, p. 94; Secchi: *le Soleil*, I p. 60; Perry: Lockyer's *Chemistry of Sun*, p. 406.

† Trouvelot: *Amer. Jour. Sci.*, March, 1876; Secchi: *le Soleil*, I, p. 113, 58; Young: *General Astronomy*, p. 189.

§4.

EXPLANATION OF THE SPOTS AND FACULÆ.

The spots are holes in the cloudy photospheric layer, channeled out in a place when the combination of previously dissociated molecules produces there an "eruption of heat." This heat will then cause a local vaporization of the cloudy condensed matter and will thus produce a hole, which will be seen as an obscure spot, not only because the cloudy matter, which was there the sole source of white light, will have disappeared, but also because this matter, without, however, changing in temperature, will then be transformed into a considerable mass of absorbing gas, which will prevent the light from the deeper and hotter layers from reaching us. This luminous absorption in the spot cavity will produce, however, only a relative obscurity. For the white brilliancy of the deeper layers is so intense that after the absorption above described it is still, even in the darkest part of the spots, more dazzling than the Drummond light.*

If such is the condition of the spots in our tranquil Sun, *their temperature must be equal to that of the adjacent photosphere.* I am well aware that MM. Henry, Secchi, Spörer and Langley have observed that the spots emit less heat than the other parts of the photosphere, but this by no means proves that they have a lower temperature. I see there merely the very simple effect of a smaller emissive power. And this emissive power must be smaller, for at a given temperature a gas emits less heat than a cloud of condensed particles. It is for the same reason that the flame of illuminating gas diminishes perceptibly in emissive power if it is converted into a Bunsen flame by introducing air. This flame, although increasing in temperature, diminishes very sensibly in radiating power, as may be perceived by the hand held at a short distance.† And if the spectra of spots show an increase in the intensity of several absorption lines, this does not demonstrate that the temperature has been lowered, for such an increase in intensity will be observed if, the temperature remaining unchanged, the absorbing gas is (as my theory requires) greatly increased in mass.

The spots are thus in the photosphere what pools of water are in a layer of melting ice. This water, though formed by heat, does not differ in temperature from the surrounding ice. In the

* Young: *The Sun*, p. 125; Leverrier: *Compt. rend.*, 8th Feb., 1869, p. 319.
† Tyndall: *Heat a Mode of Motion*, p. 498.

spots and the other parts of the photosphere there are thus only differences of latent heat, which consequently leave the masses in relative repose.

The photospheric matter, when vaporizing to form a spot, while not changing in temperature, increases very considerably in volume. It will thus cause a pressure, which while aiding to render the spot conical, on account of the less resistance of the upper level, must crowd back the surrounding clouds and will give rise to the increase of level and brightness which is ordinarily observed in the edge of a spot? For this reason aspot without faculæ is as rarely found as a terrestrial valley without neighboring hills. This increase of level will moreover, be more marked on the side where the spot is moved by the rotation of the Sun than on the opposite side; for the motion of the expanding gas being on the first-mentioned side contrary to the rotary motion, must produce there a stronger compression. Does not the observation of Warren de la Rue, that, in general, the spots which are seen on the eastern side of the solar disk seem larger than those which appear on the opposite side, find a simple explanation in this crowding of the eastern edge of the spot? We also know that the photographic negatives obtained at Kew by MM. de la Rue, B. Stewart and Loewy have rendered evident this greater developement of the faculæ on the following side of the spot, where they sometimes even form veritable tails of faculæ.*

If my theory of the spots easily explains the formation of the faculæ which surround them it also accounts for the faculæ which seem to precede them, and which, studied especially by Secchi, Spoerer and quite recently by Mr. Sidgreaves, may be considered as *precursory faculæ*.† Such faculæ will be formed (1) by the raising of the level, which must be caused in the upper photospheric layers by every formation of gas in the deeper layers; (2) by an increase in a given place of the condensation in the adjoining chromospheric and photospheric layers, a condensation which according to my theory is necessary to prepare for a future "eruption of heat." Such also seems to be the origin of the facular net-work which Mr. Hale has discovered.‡

* B. Stewart: *Proc. Roy. Soc.*, 13 p. 168; Secchi: *le Soleil* I. p. 165; De la Rue, B. Stewart and Lœwy: *Proc. Roy. Soc.* 14 p. 39.

† Samter: *Himmel u. Erde*, I, p. 44; Sidgreaves: *Monthly Notices*, Dec. 1891; Meyer: *Die Kinder der Sonne*, 1891, p. 15; Sidgreaves: *Astronomy and Astro-Physics*, 1892, p. 212.

‡ The faculæ which according to Professor Hale cover the entire photosphere would appear to be prominences in process of formation, *i. e.*, prominences which are still white, such as one sees during an eclipse. Concerning these white prominences we shall have something to say in the following section. Hale: *Astronomy and Astro-Physics*, 1892, p. 415.

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My theory also explains the various motions shown by the spots. They owe their different angular velocities in different latitudes to the cloudy zones in which they are borne. If they frequently move a little more rapidly than these clouds it is because the gas, which in the growing spot pushes back the matter of the photosphere, must move particularly toward the side where the resistance is least. Now as this side is that of the foremost edge of the spot "every time a spot undergoes sudden changes *it ordinarily advances on the solar surface by making a sort of leap.*"* This leap will also take place when, as the gaseous contents of a spot are recondensing, the vacuum thus produced will draw in again the surrounding photospheric matter. For this matter rushing in, preferably on the side where it already moves in the direction of suction, will fill up the spot from behind, once more giving to its center a sudden acceleration.

It will be seen likewise that the centrifugal motion impressed on the matter at the center of a nascent spot cannot remain rectilinear, but that, being quickly retarded it will deviate into spirals as soon as it has been conducted by this movement into zones of new velocities. That is why the spots show spiral forms without actual rotation. If this explanation is correct, the spirals will be seen most strongly curved at the posterior side of the spot,† at the northern and southern sides of the same spot these spirals should be curved in opposite directions,‡ and spirals will be observed especially in spots which have just finished their growth or those on the point of disappearing.§ For if the photospheric clouds, where dispersed by the vapor produced at the center of a nascent spot, are spun into spirals, they will also return into spirals when the vapor, condensed anew, shall have produced a partial vacuum into which the photospheric matter will rush from all sides.

As to the *motion of the spots in latitude*, it is a remarkable coincidence that this movement changes sign in the same parallel of about 11°,‖ where—according to my theory—the difference in velocity between the photospheric zones and their atmosphere changes sign also; it is, I say, this remarkable coincidence which I think has put me on the road to a plausible explanation. This explanation, however, needs a lengthy development and can not be summarized here.

* Young: *The Sun*; p. 110; Secchi: *le Soleil*, I, p. 141.
† See fig. 45 in Secchi's *le Soleil*, I, p. 88.
‡ Young; *The Sun*, p. 67.
§ Secchi: *le Soleil*. I, p. 88.
‖ Carrington: *Obs. of the Spots of the Sun*; Secchi: *le Soleil*, I, p. 121; Young: *The Sun*, p. 110; Spoerer: *Publ. Astr. Obs. Potsdam*, IV, 4, p. 418.

The separation of spots into definite zones has already been briefly explained in the section preceding. I have there explained what, according to my theory, is the *principal cause* of that distribution. But my theory requires several other causes which have been developed at length in my above mentioned memoir.

The change in the spectra of spots which Lockyer has discovered, namely, that at the approach of a minimum they show more and more distinctly the well-known lines of the terrestrial metals,* is, in reality foreseen from the diminution in latitude which they then show. For the photospheric zones where the spots are hollowed out float in the equatorial enlargement of the solar gas, and consequently contain the principal metals which, in virtue of their specific gravity, at higher latitudes remain buried in the depths. In comparing (1) the table in which Lockyer has so clearly shown that 1879 to 1880 there were sudden appearances of new lines, and (2) the table in which Spoerer has also very plainly shown that in the same years the spots also completely changed changed in latitude,† it is immediately seen that these two simultaneous changes are precisely what my theory demands.

My explanation of the periodicity in the number of spots can be *epitomized* as follows: while in the photospheric gas the molecules R, considered above, are still very numerous, there must be an exceptional combination of favorable circumstances for the molecules A and B to be able to produce a "heat eruption" of any account. That is the minimum period. But when after a long continued condensation of the molecules R, which are in the neighborhood of the photosphere layer, the molecules A and B, at first being much scattered, cease to be sufficiently separated and commence in turn an active combination. Then comes the period of incessant heat eruptions and the maximum period.

The molecules A and B by reason of this combination may lose the energy which they have; but other molecules, A′ and B′, etc., will replace them perhaps. But these in turn will exhaust themselves. Thus new spots will ever succeed one another without cessation, and we would wait in vain for a return of the minimum period were there not in the photospheric layers molecules, P and Q, with a stronger affinity, which await only condensation and rapid effacement of the molecules still separating them, to

* Lockyer: *Chem. of Sun*, p. 313-325.

† Lockyer: *loc. cit.*, pp. 318, 325. The perfect agreement between the observations of Lockyer and the predictions of my theory give strong support to the view that the surrounding gaseous layers have curvatures different from that of the photosphere. If this hypothesis be correct this gas should have, in each latitude, a different chemical constitution. It is for this reason that the equatorial spots and prominences show so many metallic lines.

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produce in turn their heat eruptions. These eruptions are then powerful agents of destruction to the molecules AB newly formed and restore as much as possible to its former state the composition which existed at the preceding minimum.

If this is a correct explanation, the periodicity of the spots depends in the first place on the molecules P + Q + A + B + R, of which the photospheric matter which produces the spots is composed. But as this matter has, according to my theory, at each latitude a somewhat different composition, the spots also should have at each latitude a somewhat different period. This is a conclusion which the observations of Spörer have perfectly confirmed. It is only necessary to consult the tables which Spörer has published showing the relation between the frequency of spots and their respective latitudes during the ten epochs into which he has divided each eleven-year period.* These tables show very clearly that when the maximum occurs at 16°, the maximum at 30° has already taken place two years previously, while that at 5° takes place two or three years later. They show in addition that the variations in the different local activities differ not only in phase, but also in amplitude and duration.

If we speak nevertheless of a periodicity in general of the solar activity, that periodicity has *no reference to any activity of the solar mass as a whole*, but is simply the resultant of all the local activities of different phases producing in *the total number of spots*, a maximum when their *mean latitude* is about 16°. The most of the faculæ, eruptive protuberances, and coronal rays being, as we shall presently see, *the effect of pre-existing spots*. When the Sun has a maximum of spots, it will evidently have at the same time a maximum of all those other marks of its activity.

§ 5.

Explanation of the Prominences.

The prominences are the transient "luminescences" at a place where in the quiet atmosphere of the Sun, elements hitherto dissociated are combining in proportion as their continual loss of heat makes this possible.

In order to make clear just how the diverse phenomena which these prominences present completely agree with the above definition, I must recall that if the elements A and B are combining in the prominences, that combination will take place only when

* Spoerer: *loc. cit.*, p. 411-415.

the cooling shall have sufficiently condensed the molecules R which separate them; and that, this combination having taken place, *the "heat eruption"* then produced will separate anew the molecules R, recently condensed, and give them again the power of hindering the combination of the rest of the molecules A and B.

If such is the chemical process which causes the prominences, they should then show intermittent condensations. Now this is a requirement of theory corresponding, in fact, to observations. For in observing prominences during an eclipse, some of them appear to be made up of a white, dust-like material with a continuous spectrum. Among the white prominences which are then seen, there are some which the spectroscope completely fails to show, and that part of the ordinary prominences which the spectroscope does show us is, so to speak, only the skeleton. The white prominences have already been observed by Littrow, Carrington and Liais, who have also commented upon its fleeting character. It has especially been studied by Tacchini and Handrikof (August, 1887). Swift also made a careful study of it during the eclipse of Jan. 1, 1889.*

There are besides many other phenomena which prove that, in the solar atmosphere, these intermittent flashes of a white matter are not at all rare. I will cite here only (1st) the *"violes roses"* which Secchi has observed in the interior of spots and which are, no doubt, caused, as he thinks, by *the visible transformation of a white preexistent matter*;† and (2nd), the faculæ which Hale, by the use of his spectroheliograph, has seen to cover in 27 minutes the spot of July 15, 1892, faculæ which an hour later, had completely disappeared.‡

If the white glow of the prominences (and above all its intermittent character) can be considered as a striking verification of my theory, *their rose or peach-blossom color* is not unpredicted.

* Tacchini: *Rapp. d. eclisse del* 1870, tav. 5; Secchi: *le Soleil*, II. p. 78, I. pp. 373-376; P. Kemp: *Himmel u. Erde*, October, 1889, p. 35; Clerke: *Hist. Astron.*, p. 248; Hastings: *Amer. Jour. Sci.*, Jan., 1891; Lockyer: *Chem. Sun*, p. 407; Hale: *Astr. Nach.* 3053; Fényi: *Publ. Haynald Obs.* VI, 1892. *Uber die im Spectroscop imsichtbare Protub. vom 19 Aug. 1887*; Tacchini: *Mem. Soc. Sept. Ital.* 1889. *Sul eclisse tolate di sole del 19 Agosta, 1887*; Egoroff: *C. R.*, 109, p. 292; Argo: *Ann.* 1846, p. 460; Lockyer: *Solar Physics*, p. 108; Klein: *Die Sonnensystem*, p. 36, etc.; Stoney: *Phil. Mag.*, 1868, p. 450; Liais: *l'Espace celeste*, p. 55. The opacity of prominences seems to indicate that they are composed of matter in a sort of nebulous state. See Secchi: *le Soleil*, II, p. 239. Hale: *Astronomy and Astro-Physics*, 1892, pp. 415, 431, 814; W. H Pickering: *Ann. Harvard Coll. Obs.*, 18, 5, p. 100.

† Secchi: *le Soleil*, I, p. 104, figs. 53, 54, 55.

‡ *Nature* Sept. 8, 1892; Hale: *Astronomy and Astro-Physics*, 1892, p. 611; Hale: *loc. cit.*: Jan., 1893; Hale: *loc cit.* 1892, p. 431; Carrington: *Monthly Notices*, Nov. 1859.

For such a glow should result in the combination of the molecules A and B, and is in consequence a "chemical luminescence," precisely like that with which Wiedemann, R. v. Helmholtz, W. H. Julius and Pringsheim have made us familiar. It was previously believed that the brightness of a flame was principally due to its temperature. But it has been recently demonstrated that that brightness results rather from the chemical processes which take place in the flame. According to the very recent researches of Pringsheim and the earlier work of W. Siemens, the importance of those chemical processes would even be so great that without them the incandescent gases would not emit any light at all.* Now if the same gas at a constant temperature will or will not produce spectral lines, according as it is the seat of a certain chemical process or not, I recognize in this a beautiful confirmation of my theory of prominences, and the definite solution of a difficulty which I found it impossible to solve when, in 1888, I published my theory for the first time,—the difficulty of explaining *how it could be possible for the prominences to be more luminous than the surrounding parts of the solar atmosphere without being hotter.* Let us now explain why the prominences which are kindled in the region of the spots, have so often the *appearance of eruptions* and *enormous velocities.* These peculiarities are easily explained if we reflect that the spots emit less heat than does the photospheric matter which encompasses them and that, as a result, in those places above a spot, the radiation of the Sun will cause a greater loss of heat than elsewhere where that loss is more effectually prevented by the heat which is there radiated more abundantly from the undisturbed photosphere. Now as every prominence is the effect of a chemical combination, *which can be caused only by a loss of heat*, it is evident that all places above a spot will be, *ceteris paribus*, much more in position to form a prominence than neighboring localities. If then that prominence is produced it will have the deceptive appearance of an illuminated sheaf shooting out from a hole.

If the luminous displacements which these prominences exhibit often have great velocities, it is because they are suddenly caused by a cooling which is propagated with the enormous velocity of heat radiation. In the explosive mixtures which are burned in our laboratories† the chemical action is propogated in quite another manner. This propagation is relatively slow because all parts of the comparatively cold mixture must be successively

* See note of section 2.

† A. M. Clerke: *Knowledge*, Feb., 1893, p. 29.

kindled and heated by the contiguous parts already in combustion. In the prominences there is *absolutely* nothing of this kind. The chemical processes there do not require any considerable heating of a cold gas, but an infinitesimal cooling of a gas already highly heated. Now it is evident that, starting from a nascent spot this infinitesimal cooling should propogate itself with the velocity of radiant heat.

If, however, the displacements of the luminous state never attain that great velocity, it is because (as I have explained above) it is impossible, in a gaseous mixture as vaporous and as complicated as the solar gas, that the dissociated matter A + B can be always ready for combination at the least fall in temperature. It is the condensation of the molecules R, which in taking more or less time, so greatly diminishes the velocity of the luminous displacement and also causes in the prominences those queer accelerations and retardations so often observed.

There is no velocity, however small or great, which my theory does not provide for. It is only when this speed surpasses that of heat radiation that it becomes inexplicable.

It is clear, moreover, that in virtue of the difference in angular velocity between a spot and the atmosphere which covers it, new prominences of the same spot will be constantly produced in the changing atmospheric mass which, for the time being, overlies the spot. As the production of prominences requires some time, it is easily seen, 1st, *why spots often appear near an eruptive prominence** and 2d, why, above the spots observed near the limb, "*eruptive prominences are sometimes seen to renew themselves after intervals of 5 or 6 hours*, and to flame out on one side and be extinguished on the other."†

But, someone may say, if all the prominences are merely deceptive appearances, why is it then that we find *metals* at the bases of some of them and not at others? To this I reply that, by reason of the slight flattening of the solar stratified layers, the equatorial atmosphere ought to contain, at its base, metals which at higher latitudes will always remain buried in the depths of the photosphere. Now as *the prominences are always of the same material as the tranquil stratified atmosphere which they at times illuminate*, clearly those metals will be seen in them only when they rise in the deepest layers of the equatorial zones. And as it is only the eruptive prominences which rise from below, they are also the only ones to show us these metals. Their

* Secchi: *le Soleil*, II p. 177.
† Secchi; *l. c.* p. 66.

height will in this region moreover count for nothing. Even at the least height the equatorial prominences will always be metallic. It is, according to Secchi, ordinarily small eruptions which are remarkable for their richness in metallic vapors.*

I do not wish to finish with the prominences without first saying a word about the displacement which their spectral lines often show. This displacement is always interpreted as if it certainly demonstrated that we are dealing with real motions and not with a simple change of position of luminous form. But that certainly does not exist. For it has never been proved that that interpretation generally adopted is the only one possible. It is not that I doubt in the least the entire truth of Doppler's principle. That truth is definitely established; for Huggins, Zöllner, Vogel, Hastings, Young, Langley, Cornu, Crew and Dunér have clearly proved that the rotation of the Sun really causes the required displacement of the lines.

But this displacement of lines so well established when luminous matter is displaced does not at all prove, and never can, that a similar displacement of lines cannot also be caused in an entirely different manner and in a way, for example, such as that which my theory calls for.

If the well established displacement of lines when luminous matter is displaced, were a phenomenon easy of description and explanation, one would hardly dare to offer any interpretation other than that ordinarily given. But that complete explanation has never been given, and in fact to give one a very clear idea of the phenomenon it is necessary to know, *first*, the motion in the source of light which produces the waves in the ether; *second*, what, on the other hand, is the influence of the waves of ether upon the vibratory movement of the luminous atoms, and, *third*, in what manner the individual vibrations of the atoms interfere to produce waves of determinate phase.

Now all these details are still very mysterious. So long as one does not understand them, he cannot have the least certainty that a displacement of the lines is not produced just as well by the luminous propagation of a chemical synthesis in the tranquil matter as by the transport of the luminous matter itself.

If, as a result, the study of prominences proves that it is impossible for their motions to be real, the displacement of their lines will not prove that the impossible is true nevertheless.† *Let*

* Secchi: *l. c.*, p. 149; Fényi: Publ. des Haynald obs. 1892, pp. 13, 22.

† My new interpretation of the displacement of the lines at once received a favorable review at the hands of Mr. Fowler, who, in *Nature*, 21st March, 1889, says expressly that he sees no reason to differ from me in this new explanation.

no one imagine, however, that in thus establishing my theory, I have had to sacrifice Doppler's principle.* It is not the principle itself which I attack, but only its common interpretation which has never been demonstrated. For if it is proved that a displacement of the luminous source causes a displacement of lines, it does not at all follow that, conversely, each displacement of lines is *necessarily* brought about in the same manner. Consequently I sacrifice only an unproved assertion. Now it seems to me that my theory, so full of plausible explanations, is well worth so insignificant a sacrifice.

§ 6.

EXPLANATION OF THE CORONA.

The brilliant filaments, the rays, the streamers and the sheets of white light at the limb of the eclipsed Sun forming an aureole of light to which we have given the name corona, arise at those places in the gaseous envelope of the Sun where a continual loss of heat causes the condensation of a cloudy mist. This mist is incandescent and emits a white light with continuous spectrum, but as it reflects also the light of the photosphere, the resulting light is more or less polarized and shows the dark lines of Fraunhofer. The coronal gas where this mist is condensed is recognized by its bright lines even in the darkest clefts. It rotates on its axis just like the rest of the Sun.†

It is clear again that the misty matter will by preference form in the places where the heat is least intense. Such places are always found immediately over a spot, since the gas with which the cavity of the spot is filled, although at the same temperature as that of the photosphere, has vastly smaller emisssive power. It is then easy to understand why, during a maximum period of spots, the coronal rays take their rise especially from the zones where the spots most abound and thus give to the corona that characteristic form of a star with four rays. If these four rays seem to originate in a latitude greater than that of the spots, it is only because we see them indistinctly projected upon the surface of the sky. The latitude from which a ray appears to emanate is not necessarily that towards which this ray is really directed. The polar zone, for example, might be completely free

Quite recently Fizeau has given still another interpretation to the displacement of the lines. His hypothesis resembles mine in that it does not require high velocities of the luminous matter. Fizeau: *C. R.* 7th Sept., 1891; *Astronomy and Astro-Physics*, 1892, p. 126.

* Koeber: *Himmel u. Erde*, April, 1893, p. 345.

† See note sec. 3.

from coronal streamers, and yet one would still see there projected the rays produced in the region of the spots.

This explanation of the square, cruciform, corona [an explanation which I have already clearly outlined in 1888 in my first "*Essai d'une Théorie du Soleil*"]* is identical with that which Schaeberle proposed later on as a result of observations made during the eclipse of Dec. 22, 1889.† Schaberle has reached the conclusion—which is mine also—that the corona is made up of rays of luminous matter projected from the spot-zones. By sticking needles into the corresponding zones of a ball, Schaeberle has made a model which reproduces many of the diverse appearances presented by the corona. According to him† the corona of April 16 last corresponded perfectly to the requirements of his theory; it quite resembled the figure which he had predicted.‡ But if the corona of April 16 perfectly agreed with the form predicted by Schaeberle, it agreed also with mine.

Since, according to Schaeberle, the corona is neither more nor less than matter ejected through the spots (holes in the photosphere) he expects to find the same coronal structure, both at the surface of the Sun and at hundreds of thousands of miles outside.

While at the time of a spot-maximum, the corona has a tendency to take the cruciform shape mentioned above, it presents at other times appearances which are quite different, appearances not so easily explained.

This capricious character is doubtless connected with the nebulous origin of the phenomena, possibly with their intermittent nature, and with their resemblance to solar prominences. In any event, we must not forget that the spots are not uniformly distributed in the "spot-zones," and that, in *all* zones, there are "pores," "veiled spots" and faculæ, each modifying the emissive power of its own neighborhood, and thus influencing local condensation. To these effects, the form of the corona is very sensitive. Since the spots have velocities which are somewhat abnormal it is clear that the condensation which is determined by the radius vector (prolonged) of the spot will continually invade new regions of the corona. Hence those rapid changes, sometimes observable within the space of twenty minutes, to-wit an apparent wavering of the streamers.‖

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* *Essai d'une Theorie du Soleil et des Etoiles variables* (Delft, Dec., 1888), p. 44.

† Schaeberle: *Publ. Astr. Soc. Pac.*, No. 7; *Nature*, May 15, 1890.

‡ Schaeberle: *Astronomy and Astro-Physics*, May, 1893, p. 463.

§ Schaberle: *l. c.*, Jan. 1893. Plate II.

‖ Young: *The Sun*, p. 192; Lockyer: *Solar Physics*, p. 294, 377; *Nature*, Vol. 18, p. 457; Pritchett: *Rep. Wash. Univ. Eclipse Party*, Eclipse of Jan. 1, 1889; *Nature*, 24th, Dec. 1891.

From what has been said, it will be seen that the beautiful corona of the maximum-period teaches us very little as to the real form of the coronal layer. For its figure is determined very largely by the condition of the photospheric surface in very distant parts of the Sun. What this condition may happen to be at any time is for us, at present, a mere matter of chance.

The minimum corona will, of course, be more regular and less brilliant.* For the spots and faculæ are then fewer in number, and the emissivity of the solar surface does not vary sensibly from one zone to another. There is nothing, therefore, to determine a greater intensity of coronal light in one zone than in another.

It is then at minimum-period, if ever, that one may hope to see this extremely tenuous outer layer of the Sun in its true form. At such a time, above all others, photographs are important. The excellent plates which Mr. Barnard obtained on the 1st of January, 1889, are, in this respect, exceedingly valuable.

If my theory is true, the gaseous envelope of the Sun has a density much greater than has usually been assigned to it. The prevailing opinion is based upon the sharpness of the Fraunhofer† lines and upon the vanishingly small resistance which the corona offers to the passage of comets.‡ But I have shown in my "*Memoire*" published by the Amsterdam Academy (pp. 87–115) that these arguments are by no means convincing. It matters little that my discussion is too lengthy to produce here, for my views have recently received the most thorough support in Deslandre's observations during the eclipse of the 16th of April, 1893.§ His measures of the equatorial velocity of the corona at a distance from the Sun amounting to two-thirds of a solar diameter show that the corona rotates just like the rest of the Sun. Accordingly the coronal envelope is one immense atmosphere exerting pressure upon the photosphere. For it is quite impossible that an atmosphere so tremendous should have a negligibly small density from its highest, all the way down, to its lowest portions.

DELFT, 16th July, 1893.

* Lockyer: *Chem. of Sun*, p. 443; Clerke: *Hist. of Astron.*, p. 233; Pickering: *Astronomy and Astro-Physics*, May 1893.

† Scheiner: *Spectral-analyse d. Gistirne*, (1890), p. 206.

‡ Scheiner: *l. c.*, p. 207; Clerke: *History of Astronomy*, p. 249; Young: *Elements of Astronomy*, p. 147; Fényi: *Mem. Soc. Spet. Ital.*, 21, 1892. *Note sur une Protuberance excessivement grande observee le 3 Oct., 1892.*

§ Deslandres: *C. R.* 15th, May, 1893; *Nature*: 25th May, 1893.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopyand other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Is there Oxygen in the Atmosphere of the Sun?—M. J. Janssen makes the following remarks on the note of M. Dunér printed in our present number:

"In this note the learned Director of the Upsala Observatory seeks to show that the complex question of the presence of oxygen in the Sun's gaseous envelopes may be regarded as solved by the observations which he has made in the course of his researches on the rotation of the Sun.

I wish to say here that unfortunately I do not share this opinion. I have in fact been led by long and profound study of the question, to recognize that the method of displacement of lines, admirable as it is in other respects, is insufficient to give a solution of the problem. If the question of the presence or absence of the metalloid oxygen in the solar atmosphere could be solved by the simple experiment of causing the lines to oscillate, it would be useless to devote one's self to long and laborious observations at elevated stations, or with tubes full of oxygen at different pressures at the surface of the Earth.

The discrimination between the solar and the terrestrial origin of a spectral line, on the basis of changes of refrangibility due to the Sun's rotation, was made for the first time by Thollon. It constituted one of the most decisive proofs of the reality of the principle which was first enunciated by Döppler, but which, in the hands of M. Fizeau, has become an admirable method of confirming and measuring celestial motions.* In 1884, M. Cornu, in order to distinguish a line of solar from one of terrestrial origin, gave to this method an extremely elegant form, by an arrangement which gives a kind of oscillatory motion to the solar lines, and permits them to be immediately distinguished from the fixed lines of terrestrial origin.

M. Dunér, in his important researches on the rotation of the Sun, which were made much later than these investigations, has not certainly done more than confirm them.

But it should now be remarked, that if the distinction between a dark line of exclusively solar origin and one due entirely to the terrestrial atmosphere can in fact be made with certainty, because the phenomenon is there presented in all its simplicity, the case is not the same if the line is due at the same time to both the solar and terrestrial atmospheres. One might then fear that the displacement of the solar part of the line would be masked by the breadth of the terrestrial part; all the more, in the case of oxygen, because we know to-day that the action of the terrestrial atmosphere gives a very great intensity to the oxygen lines, and that therefore, a solar action, if it exists, can be only a very feeble one.

But the phenomenon is still more complex, for the question of solar oxygen necessarily involves the coronal atmosphere, and we know that this atmosphere is the seat of violent movements, about which, however, very little is known. But if dark lines originate in that atmosphere, the phenomena of change of refrangibility due to motions of the source would necessarily depend upon these

* Some observations have also been made outside of France.—*Tr.*

movements, and might in an unknown measure modify the effect of the probable rotation of the coronal atmosphere.

Thus it may be seen how complex the question is, and how inadequate to its solution is the method based on the displacement of lines. It is these considerations which have caused me to attack the question from another side.

I have thought that it would first be necessary to determine the part played by the Earth's atmosphere, and only undertake the solar phenomena after this first part of the question had been elucidated.

The instrumental equipment of the Observatory of Meudon allows an absolutely decisive experiment to be made on the origin of the groups of oxygen lines in the solar spectrum. Oxygen at a pressure of 28 atmospheres has been introduced into a steel tube 60 metres long, lined with red copper. This quantity of oxygen represents that which is contained in the terrestrial atmosphere, with respect to a ray traversing it in the direction of the zenith, and it has been proved that with a powerful electric light as luminous source, the groups, notably A and B, have an intensity comparable with that of the same groups in the solar spectrum during the summer when the Sun is very high. A phenomenon is thus presented in which it is only necessary to judge of the equality of two effects, constituting the most favorable condition that can be realized in researches of this nature.

The same experiment was made under another form in 1889, with the aid of a powerful electric light placed on the summit of the Eiffel tower. The stratum of air between the tower and the Observatory of Meudon represents quite accurately in its absorptive action, the Earth's atmosphere, and in this case also it is possible to estimate the practical equality of the effect with that of the solar spectrum.

In 1888, at the Grands-Mulets, I attacked the question under another form. My intention was to study the decrease in the intensity of the groups in question due to the altitude of the station.

These observations, even then conclusive, were repeated in 1890 at the Bosses-du-Dromadaire, and finally on the summit of Mont Blanc, with a large grating spectroscope defining the elementary lines of the groups A, B and α with great perfection; the results are known.

I believe that these researches show that the terrestrial atmosphere may be regarded as the sole cause of the presence of the oxygen groups in the solar spectrum, and that this first part of the question can only be solved by the study of the terrestrial atmosphere, where the phenomena presented are absolutely known to us. It may then be said that the gaseous envelopes of the Sun do not contain oxygen, at least in a state capable of producing the absorption phenomena which we see produced in our tubes and in the atmosphere of the Earth.

It is now necessary to attack the Sun considered by itself, and this I have already commenced to do by examining the spectrum of oxygen at a high temperature. I shall send an account of these new labors to the Academy.

To sum up; the method which has served M. Dunér in his important researches on the rotation of the Sun, researches, however, in which the author did not have in view the question of the presence of oxygen in the Sun, cannot, in my eyes, be regarded as in itself capable of elucidating the complex question which is the subject of his note."—*Comptes Rendus*, cxviii, No. 2, (8 Janvier, 1894).

While we fully recognize the importance of M. Janssen's researches on the origin of the oxygen lines in the solar spectrum, we do not think

there is much in these remarks to weaken the force of M. Dunér's argument. It is not clear what the possible existence of oxygen in the corona has to do with the matter. As both authors point out, the observations which are considered relate to only one phase of the question, namely, the existence of solar lines coinciding with the lines of the terrestrial groups A, B and α. If such lines exist it follows that there is oxygen in the Sun. If they do not exist it does not follow that there is no oxygen in the Sun. Assuming the existence of a solar component, there is no doubt that the doubling of these oxygen lines at the east or west limb of the Sun would be certain to occur, as pointed out by M. Dunér; the only question is as to the possibility of detecting it. If the difficulty of the observation lies in the faintness of the solar component, as suggested by M. Janssen, then we may point out that his own observations would be indecisive for the same reason; for on the summit of Mont Blanc more than half the mass of the Earth's atmosphere is still above the observer, and in the comparisons with tubes full of oxygen the slight additional intensity of the telluric lines in the solar spectrum, due to a true solar component, would be inappreciable.

An estimate of the difficulties arising from the closeness of the components can be formed with the aid of the following considerations. The maximum separation would be very nearly that due to the motion of the east or west limb of the Sun; at B it would amount to about 0.05 tenth-meters. This is about the limit of resolution of Rowland's map; but in his memoir on the rotation of the Sun Professor Dunér has given a list of lines not recognizable as double on the map, but which he found to be double with his instrument. Hence it would seem that by careful observation of the sharpest lines, the duplicity in question should be detected.

Professor Schuster contributes a note on the same subject to the *Comptes Rendus* (Jan. 15, 1894), in which he points out that in 1877 he showed that several lines in one of the spectra of oxygen are perhaps represented by dark lines in the solar spectrum. Absolute coincidence of the lines cannot be asserted on account of small errors to which the observations were subject, but he thinks their possible identity should be considered in a discussion of the question.

Report of the Wolsingham Observatory for the year 1893.—In this very brief report Mr. Espin does little more than mention the various important observations made by him during the past year. They relate chiefly to the discovery of stars with remarkable spectra, and the results are well known to our readers. Four hundred and eighty-nine such stars were discovered besides fifteen nebulæ not included in the New General Catalogue.

The Observatory has sustained a severe loss in the death of Miss Brook, who was greatly interested in its work and generously defrayed many of the incidental expenses.

Large Rock-salt Prism and Lenses.—Mr. Brashear is making for the Smithsonian Institution a rock-salt prism and set of lenses which, when completed, will probably be the largest optical train yet constructed of this material. The 60° prism is seven inches high, and the width of each face is five inches. The lenses are seven inches in diameter. While the prism is not perfectly free from flaws, the loss of light from this cause will hardly be more than five per cent., and the lenses are even clearer than the prism. All three pieces were cut from a block of rock-salt which formed part of the Russian mining exhibit at Chicago.

Photographic Investigations of Captain Abney.—The relation between the brightness of a photographic image and the time of exposure necessary to produce a given density in the negative is a very important one, as it has a direct bearing on the methods of determining stellar magnitudes by photography. The simple relation that the product of the intensity and the time is a constant can only be assumed to hold within certain limits, and Captain Abney has always made this reservation in stating his belief in the principle. In a recent communication to the Royal Astronomical Society (*Monthly Notices*, LIV, Dec. 1893) he admits, however, that he was not prepared to find so wide a departure from the usually accepted law as that indicated by some of his experiments. With very feeble light the time of exposure had to be much more than proportionally increased.

Another important fact brought out by the investigation is that the sum of a large number of small exposures is not equal to the same exposure given at one time.

According to Captain Abney's results a large telescope has a greater advantage over a small one, in certain kinds of photographic work, than would be expected from a comparison of the areas of their objectives. The more the subject of photographic magnitudes is investigated the more numerous seem to be the difficulties with which it is beset.

Theory of Optical Instruments by Dr. Czapski.†—The ease with which all the fundamental laws of optics lend themselves to exact symbolic expression has made the subject a tempting one to mathematicians. One result has been a rather wide gap between theory and practice, between theory and technique, perhaps we should say, from which each possibly has suffered.

The book which Dr. Czapski has given us may fairly be called remarkable for the manner in which on every page he expresses his experimental facts in easily handled equations and, conversely, interprets his equations in terms of practical experiment. In this respect the author is quite in harmony with Mascart in his *Traite d'Optique* and Lord Rayleigh in his *Wave Theory of Light*, in each of which the constant appeal is from pure mathematics to experiment.

Another striking feature of the work is its excellent balance; a sense of proportion is seldom, if ever, lost; the special case is always exhibited in its relation to the general, at least in so far as the "universal dovetailedness of things will permit."

The first fifty pages are occupied by an exceedingly general and, of course, essentially Gaussian, treatment of the subject. This, like the remainder of the book, is laid down on the lines of Abbe, and makes by far the most elegant presentation with which your reviewer is familiar.

Any optical system whatever is treated simply as a device for transforming a plane in one part of space into a corresponding plane in another part of space. For convenience, these two regions are called "object-space" and "image space" respectively. The whole of what is ordinarily known as geometrical optics is based upon this single definition. The optical instrument thus defined is completely *described* by the fifteen constants of the following equations, which determine any point (x', y', z') in the "image-space" in terms of its corresponding point (x, y, z) in the "object-space."

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† *Theorie der Optischen Instrumenten nach Abbe* von Dr. Siegfried Czapski. (Breslau, 1893.) pp. 292.

$$x' = \frac{a_1x + b_1y + c_1z + d_1}{ax + by + cz + 1}$$

$$y' = \frac{a_2x + b_2y + c_2z + d_2}{ax + by + cz + 1}$$

$$z' = \frac{a_3x + b_3y + c_3z + d_3}{ax + by + cz + 1}$$

Planes of discontinuity in these two regions are recognized later as the two focal planes. The single case in which there is no discontinuity, *i. e.*, the case in which finite values of x, y, z, always yield the finite values of x', y', z', is later seen to be the case of the astronomical telescope when adjusted for a normal eye. In like manner are derived a number of other fundamental theorems.

The next problem is to reduce, without any essential sacrifice of generality, the fifteen constants to a workable number. This is accomplished, in a really elegant manner, by the successive introduction of the actual working conditions and by choice of convenient reference axes; so that only two constants, A and B, remain, giving the equations

$$x' = \frac{A}{x}$$

$$y' = \frac{By}{x}$$

$$z' = \frac{Bz}{x}$$

Of these, curiously enough, B happens to be the first focal length of the system, and A the product of the first and second focal lengths.

The first of these equations is, at once, *Newton's Rule*; the second and third give the magnifying power, as ordinarily understood; while the longitudinal magnification $\left(\frac{dx'}{dx}\right)$ comes from the first by direct differentiation; and so on. This rough sketch will give a general notion of the method pursued throughout.

The treatment of lenses and systems of lenses follows next, and is perhaps more conventional than any other chapter in the book.

Chapter IV contains a very complete discussion of spherical aberration, including a lucid account of the correction method of Abbe. Throughout, the constant aim is, not to compute the size of the aberration circles merely, but rather to determine the effect of aberration on the defining power of the instrument This chapter and the one following, on chromatic aberration, have lost all the mathematical elegance of the preceding parts, a feature possibly inherent in the subject. However, before any detailed discussion is entered upon, the method of attack is first outlined, and then developed: whatever is said is clear. As might be expected one finds here (*pp.* 128-135) an excellent exposition of the properties of the Jena glass, and of Abbe's apochromatic lens which this glass has made possible. The color curves plotted on p. 131 are especially worthy of inspection. One wishes that still more space had been given to this subject, especially to the color and to the weathering properties of the glass.

The theory of stops (diaphragms) is considered at length in Chapter VII; also the brightness of images as dependent upon aperture. The necessity of clearly distinguishing between effective aperture and diameter of physical stop is

strongly insisted upon, for both incident and emergent beams. To those who will look through the book in vain for any discussion of the spectroscope, as such, these pages, together with those on the prism, are to be especially commended. On the other hand, disappointment must be confessed at not finding here a somewhat fuller discussion of diffraction effects determined by aperture. Reference is here intended especially to the distribution of light in the images of points and to the immediate dependence of resolving power upon this. The advantage of large objectives in offering increased light and diminished "spurious disc" is given, but no mention is made of the third advantage of these large glasses in giving a *steeper* intensity curve from the center to the edge of the "spurious disc." The proportion of the total incident light which an instrument is able to concentrate in the "spurious disc" is not touched upon. Possibly these and kindred subjects are reserved for the treatise which, in the preface, is promised from Professor Abbe.

The last hundred pages are devoted to the description of various optical instruments, including the human eye, the photographic lens, the compound microscope, the astronomical telescope and the focometer. Although descriptive, these chapters are nowhere prolix; and the reader will find that he must keep himself mentally wide-awake if he is to follow the author intelligently. For the student who has not followed the earlier pages as intelligently as he might wish, this later part of the volume is a helpful collection of special problems solved by the aid of the general principles laid down in the first part. To the microscopist and photographer the well known authority with which Dr. Czapski speaks makes this part of the work of high value.

Of the volume as a whole, it may be said that ordinary and uninteresting details have everewhere been omitted. Among *these* details, it is not our intention to include the index, which is not to be found. Will someone kindly explain the aversion which English and Continental writers have for indexes? In this case almost any other half dozen pages might have been omitted with less loss than those which would have sufficed for an index.

References to original memoirs abound in every chapter. The author's evident acquaintance with the work of Lord Rayleigh makes it all the more surprising that he should refer to Cotes' theorem regarding the magnifying power of any system as "*der Helmholtz-Lagrangesche Satz*. To Cotes and his theorem there appears to attach a peculiar fatality in virtue of which it has been necessary that he himself should be discovered successively by Newton* and Rayleigh, and that his theorem should be independently discovered three times in succession.

As to the author's style one word, at least, must be said. In the heaping of one dependent clause upon another to form gigantic sentences, he is possibly following only the genius of the German language. Any comparison with French or English must, of course be forborne. Into these long sentences are thrust interminable parentheses, often several in number necessitating as much brain labor on German syntax as is called for by the subject matter. These remarks might be quite out of place, were it not that recent scientific literature has shown the German language capable of better things.

To systematize and unify, as Dr. Czapski has done, so many original contributions to any subject is of course a valuable scientific service. HENRY CREW.

* It was Cotes of whom Newton made his classical remark, "If Mr. Cotes had lived, we should have known something."

Solar Observations at the Roman College in 1893.—The following is extracted from a letter recently received from Professor Tacchini:

On account of my trip to America, my communications are somewhat behindhand, and at present I am only able to send you an abstract of the solar observations made at the Royal Observatory of the Roman College in the second and third quarters of the year 1893.

1893.	Days of Observation.	Relative Frequency of Spots.	Relative Frequency of Days without spots.	Relative Size of Spots.	Relative Size of Faculæ.	No. of Groups per day.
April	28	29.11	0.00	110.5	99.6	7.3
May	25	23.16	0.00	95.2	89.8	7.0
June	27	28.74	0.00	101.3	63.9	6.9
July	30	26.30	0.00	138.1	94.2	7.3
August	31	44.84	0.00	227.5	111.0	10.2
September	30	30.77	0.00	141.1	134.0	6.7

During the second quarter of the year, the solar activity, with reference to the spots, showed an increase, and the frequency of the groups of spots remained nearly constant. As in the preceding quarter, veiled spots and pale faculæ were pretty frequent. In the third quarter the diurnal frequency of the true spots comes out rather small, but on account of the increase in the number of groups, the great number of large pores (trous), and the considerable size of the spots, the solar activity should be regarded as decidedly greater than in the preceding months, with a well-marked maximum in the month of August. The faculæ also were more numerous. Following are the results of observations of prominences:

1893.	Days of Observation.	Average Number.	Average Height.	Average Breadth.
			"	°
April	26	11.58	39.3	1.8
May	21	6.52	40.1	1.9
June	26	5.81	38.8	1.9
July	26	6.23	37.4	1.7
August	29	8.73	36.3	1.8
September	26	6.77	36.5	1.7

During the second quarter the phenomena of the prominences were of about the same intensity as in the first months of the year, and the only point requiring notice is the secondary maximum in the month of April. In the third quarter the prominences continued to diminish in importance, while the spots showed a considerable increase, showing that the relation between the two phenomena is not so intimate as it was once supposed to be. It seems appropriate to remark, in connection with this subject, that as auroras and great magnetic disturbances have been much less frequent during the period in question, my former views have been confirmed, namely; that terrestrial phenomena are more closely related to the phenomena of the solar atmosphere than to the spots. The greatest heights reached by prominences were as follows:

	′		″
April	187	July	75
May	127	Aug.	121
June	90	Sept.	121

P. TACCHINI.

The following letter has been received from Professor Tacchini:

I take pleasure in sending you the results which I have obtained for the distribution in latitude of solar phenomena, from observations made at the Royal Observatory of the Roman College during the first half of the year 1893:

1893. Latitude.	Protuberances. First Quarter.		Faculæ. First Quarter.		Spots. First Quarter.		Eruptions. First Quarter.	
90 + 80	0.000							
80 + 70	0.005							
70 + 60	0.017							
60 + 50	0.017							
50 + 40	0.052	0.339	0.013		0.000			
40 + 30	0.055		0.048		0.076			
30 + 20	0.085		0.117	0.429	0.272	0.435		
20 + 10	0.068		0.160		0.087			
10 + 0	0.040		0.091					
0 — 10	0.046		0.147		0.109			
10 — 20	0.084		0.255		0.337			
20 — 30	0.095		0.143	0.571	0.109	0.565		
30 — 40	0.127		0.022		0.011			
40 — 50	0.127	0.661	0.004					
50 — 60	0.111							
60 — 70	0.066							
70 — 80	0.003							
80 — 90	0.002							
	Second Quarter.		Second Quarter.		Second Quarter.		Second Quarter.	
90 + 80	0.000							
80 + 70	0.000							
70 + 60	0.011							
60 + 50	0.011							
50 + 40	0.045	0.345	0.000					
40 + 30	0.092		0.025		0.000			
30 + 20	0.052		0.082	0.385	0.000	0.408		
20 + 10	0.062		0.155		0.229		0.500	
10 + 0	0.022		0.123		0.079		0.250	1.000
0 — 10	0.102		0.129		0.071		0.125	
10 — 20	0.113		0.234		0.314		0.125	
20 — 30	0.094		0.186	0.615	0.200	0.529		
30 — 40	0.102		0.060		0.007			
40 — 50	0.056	0.655	0.006					
50 — 60	0.075							
60 — 70	0.108							
70 — 80	0.005							
80 — 90	0.000							

The frequency of the protuberances is nearly twice as great in the southern zones as it is in the northern, and all the solar phenomena were more frequent in the southern hemisphere; the same circumstance is manifest in the data of each month. The absolute maxima by zones was also always found in this hemisphere. The maxima of faculæ and of spots occurred in the same zones (±10°, ± 20°), while those of the protuberances were situated in higher latitudes.

In the first quarter of the year no eruptions were observed.

P. Tacchini.

PLANET NOTES FOR APRIL.

H. C. WILSON.

Mercury will be "morning star" during April, and will be at greatest elongation, west from the Sun 27° 40′, on the tenth of the month. Mercury will be in conjunction with the Moon April 3 at $5^h\ 37^m$ P. M. central time.

Venus is also "morning star" and is nearing greatest elongation west from the Sun. The greatest distance from the Sun, 46° 10′, will be reached on the morning of April 27. This will be a favorable month, so far as position is concerned, for the study of the surface markings of Venus, although the fact that she is only visible in the morning will be a drawback to all but the most enthusiastic amateurs. On the morning of April 5 Venus will be near the star α Aquarii, conjunction in right ascension occurring at $2^h\ 17^m$ A. M. central time. Venus will then be 19′ south of the star. The illuminated portion of her disc will increase during the month from one third to one half, while her brilliancy will decrease in the ratio of 195 to 139.

Mars improves a little in position during April, but it will not yet pay to spend much time in trying to observe this planet. He will move eastward and northward through the center of the constellation Capricornus. As he is brighter than any of the stars in the constellation it would not be difficult to identify him without the ruddy color which makes him so conspicuous. Mars will be in conjunction with the Moon April 29 at 1 A. M.

Jupiter will be pretty low in the west during the observing hours of April, but some satisfactory views may yet be obtained. He is moving slowly eastward south of the Pleiades. Jupiter will be in conjuction with the Moon, 5° south, April 9 at 5 A. M.

Saturn and *Spica* (α Virginis) make a fine pair in the south in the morning. They are nearly equal in brilliancy but differ a little in color, Saturn having a golden hue while Spica is bluish white. Saturn is retrograding, that is moving westward, and at the end of April will be almost directly north of Spica. He will be at opposition April 11 at noon. The moon will pass by Saturn, 4° to the south, April 10 at $9^h\ 28^m$ P. M.

Uranus is toward the southeast from Saturn in the constellation Libra. On the morning of the 27th at $7^h\ 11^m$ he will be in conjunction with the second magnitude star α Librae, being only 4′ north of the brighter component of that star which is a wide double. The motion of Uranus is so slow that he will be in the vicinity of the star for several days, so that this will be an excellent opportunity for the amateur to be sure that he has seen this planet. Note the green color and the visibility of a definite disc.

Neptune may be observed in the early evening but has past the most favorable position. He is about half way between τ and ε in the constellation Taurus.

Planet Tables for April.

[The times given are local time for Northfield. To obtain Standard Times for Place in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Apr. 5	23 21.7	− 5 35	4 47 A. M.	10 25.9 A. M.	4 05 P. M.
15	23 58.8	− 2 52	4 31 "	10 23.5 "	4 16 "
25	0 49.0	+ 2 20	4 22 "	10 34.4 "	4 47 "

VENUS.

Apr. 5	22 12.1	− 8 36	3 47 A. M.	9 16.6 A. M.	2 46 P. M.
15	22 44.8	− 6 53	3 34 "	9 09.7 "	2 46 "
25	23 20.6	− 4 22	3 20 "	9 06.1 "	2 52 "

MARS.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Apr. 5	20 33.6	− 19 56	2 58 A. M.	7 38.3 A. M.	12 19 P. M.
15	21 02.7	− 18 14	2 40 "	7 28.0 "	12 16 "
25	21 31.1	− 16 18	2 20 "	7 17.1 "	12 14 "

JUPITER.

Apr. 5	3 54.5	+ 19 43	7 32 A. M.	2 58.0 P. M.	10 24 P. M.
15	4 02.9	+ 20 09	6 59 "	2 27.0 "	9 55 "
25	4 11.9	+ 20 35	6 26 "	1 56.5 "	9 27 "

SATURN.

Apr. 5	13 26.5	− 6 06	6 49 P. M.	12 28.3 A. M.	6 07 A. M.
15	13 23.6	− 5 48	6 05 "	11 46.1 P. M.	5 27 "
25	13 20.8	− 5 32	5 22 "	11 04.0 "	4 46 "

URANUS.

Apr. 5	14 48.4	− 15 47	8 51 P. M.	1 49.9 A. M.	6 49 A. M.
15	14 46.9	− 15 40	8 09 "	1 09.1 "	6 09 "
25	14 45.3	− 15 33	7 28 "	12 28.2 "	5 28 "

NEPTUNE.

Apr. 5	4 40.0	+ 20 41	8 12 A. M.	3 43.3 P. M.	11 14 P. M
15	4 41.0	+ 20 44	7 34 "	3 05.1 "	10 36 "
25	4 42.3	+ 20 46	6 56 "	2 27.0 "	9 58 "

THE SUN.

Apr. 5	0 58.7	+ 6 16	5 34 A. M.	12 02.6 P. M.	6 31 P. M.
15	1 35.5	+ 9 57	5 17 "	11 59.9 A. M.	6 43 "
25	2 12.8	+ 13 22	5 00 "	11 57.8 "	6 55 "

THE MOON.

Apr. 1	21 26.4	− 19 30	4 03 A. M.	8 46.6 A. M.	1 39 P. M.
3	23 03.0	− 8 40	4 35 "	10 15.2 "	4 08 "
5	0 38.8	+ 4 12	5 21 "	11 42.8 "	6 19 "
7	2 22.6	+ 16 52	6 02 "	1 18.4 P. M.	8 51 "
9	4 22.8	+ 26 06	7 01 "	3 10.5 "	11 33 "
11	6 36.6	+ 28 28	8 43 "	5 16.0 "	1 45 A. M.
13	8 45.5	+ 22 53	11 12 "	7 16.6 "	3 06 "
15	10 37.5	+ 11 51	1 50 P. M.	9 00.5 "	3 54 "
17	12 17.5	− 1 19	4 19 "	10 32.4 "	4 32 "
19	13 55.1	− 13 51	6 42 "	12 01.9 A. M.	5 10 "
21	15 38.2	− 23 27	9 04 "	1 36.8 "	6 02 "
23	17 28.7	− 28 16	11 16 "	3 19.1 "	7 20 "
26	19 20.3	− 27 22	12 57 A. M.	5 02.6 "	9 11 "
28	21 05.0	− 21 18	2 04 "	6 39.2 "	11 23 "
30	22 41.6	− 11 17	2 48 "	8 07.6 "	1 38 P. M.

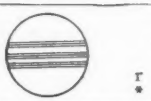
Approximate Central Standard Times when the Great Red Spot will cross the Central Meridian of Jupiter.

	h m		h m		h m
Apr. 1	7 26 P. M.	Apr. 11	5 40 P. M.	Apr. 21	3 54 P. M.
2	3 17 "	12	11 27 "	22	9 41 "
3	9 04 "	13	7 18 "	23	5 32 "
4	4 55 "	14	3 09 "	24	11 19 "
5	10 42 "	15	8 56 "	25	7 10 "
6	6 33 "	16	4 47 "	26	3 02 "
7	2 24 "	17	10 34 "	27	8 48 "
8	8 11 "	18	6 25 "	28	4 40 "
9	4 02 "	19	2 16 "	29	10 26 "
10	9 49 "	20	8 03 "	30	6 18 "

XUM

Jupiter's Satellites for April.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I.		III.	
II.		IV.	No Eclipse.

Configuration at 7h for an Inverting Telescope.

Day.	West	East.
1	○1· 4· ·3 ·2 ○	
2	4· .○3 ·1 ·2	
3	·4 1· ○2· ·3	
4	·4 2· ○ ·1 3·	
5	·4 ·1 ○ 3· ·2●	
6	3· ·4 ○ ·1 2·	
7	3· 2· ·1 ○ ·4	
8	·3 ·2 ○1· ·4	
9	·3 ○ ·2 ·4 ·1●	
10	1· ○ 2· ·3 ·4	
11	2· ○ ·1 ·3 4·	
12	1· ○ 3· 4· ·2●	
13	3· ○ 1· 2· 4·	
14	3· 2· ·1 ○ 4·	
15	·3 ·2 4· ○ 1·	
16	4· ·3 ·1○ ·2	
17	·4 1·○ 2· ·3	
18	4· 2· ○ ·1 ·3	
19	·4 1· ·2 ○ 3·	
20	○3· ·4 ○ ·1 ·2	
21	·4 3· ·1 2· ○	
22	·3 ·4 ·2 ○ 1·	
23	·3 ·1 ·4 ○ 2·	
24	○1· ○ ·32· ·4	
25	2· ○ ·1 ·3 ·4	
26	1· ·2 ○ 3· ·4	
27	○ 3· ·1 ·2 ·4	
28	○2· 3· 1· ○ 4·	
29	3· ·2 ○ 1· 4·	
30	·3 ·1 ○ ·2 4·	

Phenomena of Jupiter's Satellites.

Central Time.

		h	m			
Apr.	1	5	54	P. M.	I	Tr. In.
		6	54	"	I	*Sh. In.
		8	08	"	I	*Tr. Eg.
		9	07	"	I	Sh. Eg.
	2	2	01	A. M.	II	Oc. Dis.
		6	21	"	II	Ec. Re.
		3	16	P. M.	I	Oc. Dis.
		6	26	"	I	*Ec. Re.
		7	24	"	III	*Oc. Dis.
		9	37	"	III	Oc. Re.
		11	32	"	III	Ec. Dis.
	3	1	24	A. M.	III	Ec. Re.
		12	25	P. M.	I	Tr. In.
		1	23	"	I	Sh. In.
		2	38	"	I	Tr. Eg.
		3	36	"	I	Sh. Eg.
		9	14	"	II	Tr. In.
		11	10	"	II	Sh. In.
		11	40	"	II	Tr. Eg.
	4	1	34	A. M.	II	Sh. Eg.
		9	46	"	I	Oc. Dis.
		12	54	P. M.	I	Ec. Re.
	5	6	55	A. M.	I	Tr. In.
		7	51	"	I	Sh. In.
		9	08	"	I	Tr. Eg.
		10	05	"	I	Sh. Eg.
		3	25	P. M.	II	Oc. Dis.
		7	39	"	II	*Ec. Re.
	6	4	16	A. M.	I	Oc. Dis.
		7	23	"	I	Ec. Re.
		9	41	"	III	Tr. In.
		11	54	"	III	Tr. Eg.
		1	31	P. M.	III	Sh. In.
		3	36	"	III	Sh. Eg.
	7	1	25	A. M.	I	Tr. In.
		2	20	"	I	Sh. In.
		3	38	"	I	Tr. Eg.
		4	34	"	I	Sh. Eg.
		10	38	"	II	Tr. In.
		12	29	P. M.	II	Sh. In.
		1	04	"	II	Tr. Eg.
		2	53	"	II	Sh. Eg.
		10	46	"	I	Oc. Dis.
	8	1	52	A. M.	I	Ec. Re.
		7	55	P. M.	I	*Tr. In.
		8	49	"	I	Sh. In.
		10	09	"	I	Tr. Eg.
		11	03	"	I	Sh. Eg.
	9	4	49	A. M.	II	Oc. Dis.
		8	58	"	II	Ec. Re.
		5	16	P. M.	I	Oc. Dis.
		8	21	"	I	*Ec. Re.
		11	48	"	III	Oc. Dis.
	10	2	02	A. M.	III	Oc. Re.
		3	33	"	III	Ec. Dis.
		5	26	"	III	Ec. Re.
		2	25	P. M.	I	Tr. In.
		3	18	"	I	Sh. In.
		4	39	"	I	Tr. Eg.
		5	31	"	I	Sh. Eg.
	11	12	02	A. M.	II	Tr. In.

		h	m			
Apr.	11	1	47	A. M.	II	Sh. In.
		2	28	"	II	Tr. Eg.
		4	11	"	II	Sh. Eg.
		11	47	"	I	Oc. Dis.
		2	50	P. M.	I	Ec. Re.
	12	8	56	A. M.	I	Tr. In.
		9	47	"	I	Sh. In.
		11	09	"	I	Tr. Eg.
		12	00	M.	I	Sh. Eg.
		6	13	P. M.	II	*Oc. Dis.
		10	17	"	II	Ec. Re.
	13	6	17	A. M.	I	Oc. Dis.
		9	19	"	I	Ec. Re.
		2	05	P. M.	III	Tr. In.
		4	19	"	III	Tr. Eg
		5	31	"	III	Sh. In.
		7	38	"	III	*Sh. Eg
	14	3	26	A. M.	I	Tr. In.
		4	16	"	I	Sh. In.
		5	39	"	I	Tr. Eg.
		6	29	"	I	Sh. Eg.
		1	26	P. M.	II	Tr. In.
		3	05	"	II	Sh. In.
		3	52	"	II	Tr. Eg.
		5	30	"	II	Sh. Eg.
	15	12	47	A. M.	I	Oc. Dis.
		3	47	"	I	Ec. Re.
		9	56	P. M.	I	Tr. In.
		10	44	"	I	Sh. In.
	16	12	10	A. M.	I	Tr. Eg.
		12	58	"	I	Sh. Eg.
		7	37	"	II	Oc. Dis.
		11	35	"	II	Ec. Re.
		7	18	P. M.	I	*Oc. Dis.
		10	16	"	I	Ec. Re.
	17	4	13	A. M.	III	Oc. Dis.
		6	28	"	III	Oc. Re.
		7	34	"	III	Ec. Dis.
		9	28	"	III	Ec. Re.
		4	26	P. M.	I	Tr. In.
		5	13	"	I	Sh. In.
		6	40	"	I	*Tr. Eg.
		7	27	"	I	*Sh. Eg.
	18	2	50	A. M.	II	Tr. In.
		4	24	"	II	Sh. In.
		5	17	"	II	Tr. Eg.
		6	48	"	II	Sh. Eg.
		1	48	P. M.	I	Oc. Dis.
		4	45	"	I	Ec. Re.
	19	10	57	A. M.	I	Tr. In.
		11	42	"	I	Sh. In.
		1	11	P. M.	I	Tr. Eg.
		1	55	"	I	Sh. Eg.
		9	02	"	II	Oc. Dis.
	20	12	54	A. M.	II	Ec. Re.
		8	18	"	I	Oc. Dis.
		11	14	"	I	Ec. Re.
		6	32	P. M.	III	*Tr. In.
		8	48	"	III	Tr. Eg.
		9	34	"	III	Sh. In.
		11	41	"	III	Sh. Eg.

Apr. 21	5	27	A. M.	I	Tr. In.
	6	11	"	I	Sh. In.
	7	41	"	I	Tr. Eg.
	8	24	"	I	Sh. Eg.
	4	15	P. M.	II	Tr. In.
	5	42	"	II	Sh. In.
	6	41	"	II	*Tr. Eg.
	8	07	"	II	Sh. Eg.
22	2	49	A. M.	I	Oc. Dis.
	5	43	"	I	Ec. Re.
	11	57	P. M.	I	Tr. In.
23	12	39	A. M.	I	Sh. In.
	2	11	"	I	Tr. Eg.
	2	53	"	I	Sh. Eg.
	10	27	"	II	Oc. Dis.
	2	13	P. M.	II	Ec. Re.
	9	19	"	I	Oc. Dis.
24	12	11	A. M.	I	Ec. Re.
	8	40	"	III	Oc. Dis.
	10	55	"	III	Oc. Re.
	11	34	"	III	Ec. Dis.
	1	30	P. M.	III	Ec. Re.
	6	28	"	I	*Tr. In.
	7	08	"	I	*Sh. In.
	8	42	"	I	Tr. Eg.
	9	22	"	I	Sh. Eg.
25	5	39	A. M.	II	Tr. In.
	7	00	"	II	Sh. In.
	8	06	"	II	Tr. Eg.
	9	25	"	II	Sh. Eg.
	3	49	P. M.	I	Oc. Dis.
Apr. 25	26	40	P. M.	I	*Ec. Re.
	26	12	58	"	I	Tr. In.
		1	37	"	I	Sh. In.
		3	12	"	I	Tr. Eg.
		3	50	"	I	Sh. Eg.
		11	51	"	II	Oc. Dis.
27		3	31	A. M	II	Ec. Re.
		10	20	"	I	Oc. Dis.
		1	09	P. M.	I	Ec. Re.
		11	00	"	III	Tr. In.
28		1	16	A. M.	III	Tr. Eg.
		1	34	"	III	Sh. In.
		3	43	"	III	Sh. Eg.
		7	29	"	I	Tr. In.
		8	06	"	I	Sh. In.
		9	43	"	I	Tr. Eg.
		10	19	"	I	Sh. Eg.
		7	04	P. M.	II	*Tr. In.
		8	18	"	II	Sh. In.
		9	31	"	II	Tr. Eg.
		10	43	"	II	Sh. Eg.
29		4	50	A. M.	I	Oc. Dis.
		7	38	"	I	Ec. Re.
30		1	59	"	I	Tr. In.
		2	34	"	I	Sh. In.
		4	13	"	I	Tr. Eg
		4	48	"	I	Sh. Eg.
		1	17	P. M.	II	Oc. Dis.
		4	50	"	II	Ec. Re.
		11	21	"	I	Oc. Dis.
May 1		2	06	A. M.	I	Ec. Re.

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc. denotes occultation; Tr., transit of the satellite; Sh., transit of the shadow; * Visible at Washington.

Annular Eclipse of the Sun, April 5, 1894.—This will not be visible in the United States. The path of the annular eclipse passes from a point in the Persian Gulf, across Hindostan and China, along the east coast of Siberia, ending in Alaska. It will be visible as a partial eclipse throughout Asia, north-eastern Europe and parts of the Indian and Pacific Oceans.

ELEMENTS OF THE ECLIPSE.

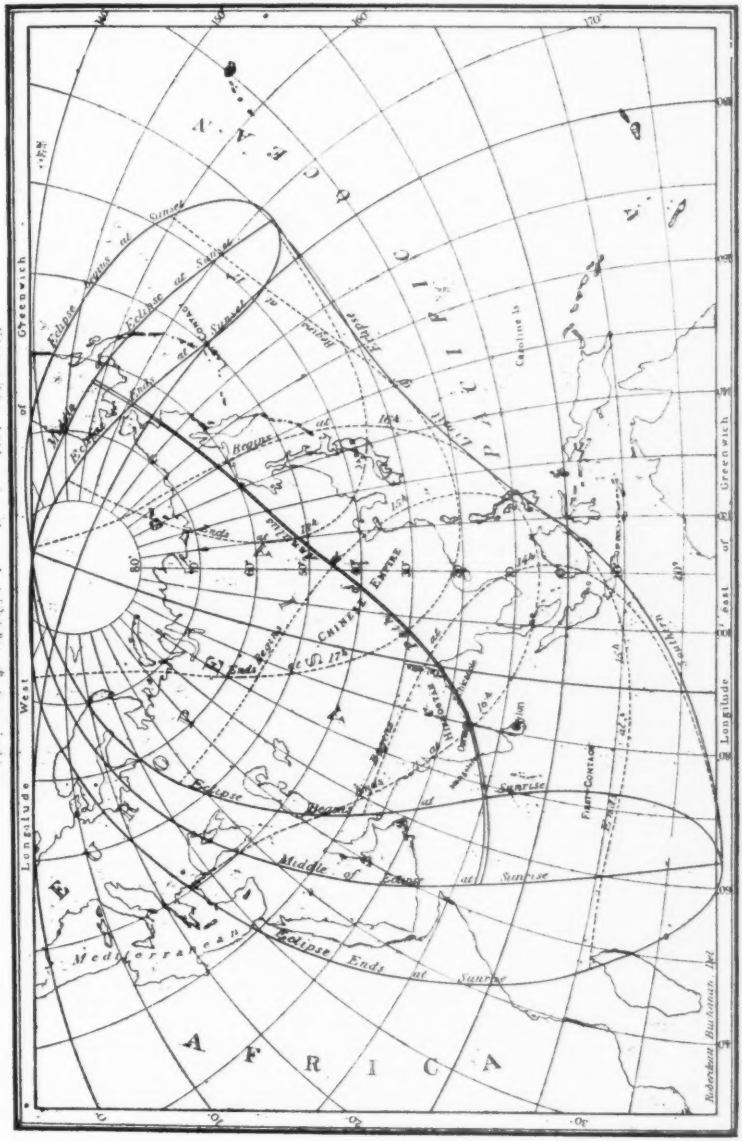
Greenwich mean time of conjunction in right ascension
April 5, $16^h \cdot 27^m \ 39^s.2$

Sun and Moon's R. A.	$1^h \ 00^m \ 16^s.90$	Hourly motions	$9^s.14$ and $121^s.28$
Sun's declination	6° 26′ 11″.6 N.	Hourly motion	0′ 56″.7 N.
Moon's declination	7 03 48 .5 N.	Hourly motion	15 53 .7 N.
Sun's equa. hor. par.	8 .6	Sun's true diameter	15 58 .6
Moon's equa. hor. par.	57 52 .5	Moon's true diameter	15 45 .5

CIRCUMSTANCES OF THE ECLIPSE.

	Greenwich Time.	Long. from Greenwich.	Latitude.
Eclipse begins April 5,	$13^h \ 15^m.9$	72° 24′.2 E.	6° 33′.6 S.
Central eclipse begins	14 24 .0	53 51 .8 E.	6 47 .4 N.
Central eclipse at noon	16 27 .7	113 42 .5 E.	47 22 .3 N.
Central eclipse ends	17 23 .3	157 30 .7 W.	62 47 .5 N.
Eclipse ends	18 31 .5	179 34 .2 W.	49 44 .5 N.

ANNULAR ECLIPSE OF APRIL 5TH 1894.



Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc.]

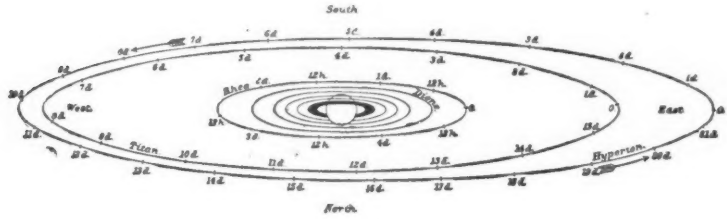


DIAGRAM OF THE APPARENT ORBITS OF SATURN'S SATELLITES.

MIMAS.

		h		
Apr.	4	2.7	A. M.	E
	5	1.3	"	E
	5	11.9	P. M.	E
	6	10.5	"	E
	7	9.1	"	E
	8	7.7	"	E
	9	6.3	"	E
	10	4.9	"	E
	11	3.5	"	E
	12	2.9	A. M.	W
	13	1.5	"	W
	14	12.1	"	W
	14	10.8	P. M.	W
	15	9.4	"	W
	16	8.0	"	W
	17	6.6	"	W
	18	5.2	"	W
	19	3.8	"	W
	20	3.2	A. M.	E
	21	1.8	"	E
	22	12.4	"	E
	22	11.0	P. M.	E
	23	9.6	"	E
	24	8.2	"	E
	25	6.8	"	E
	26	5.4	"	E
	27	4.0	"	E
	28	2.6	"	E
	29	2.0	A. M.	W
	30	12.6	"	W
	30	11.3	P. M.	W

ENCELADUS.

		h		
Apr.	2	12.3	P. M.	E
	3	9.1	"	E
	5	6.0	A. M.	E
	6	2.9	P. M.	E
	7	11.8	"	E

ENCELADUS Cont.

		h		
Apr.	9	8.7	A. M.	E
	10	5.5	P. M.	E
	12	2.4	A. M.	E
	13	11.3	"	E
	14	8.2	"	E
	16	5.1	A. M.	E
	17	1.9	P. M.	E
	18	10.8	"	E
	20	7.7	A. M.	E
	21	4.6	P. M.	E
	23	1.5	A. M.	E
	24	10.3	"	E
	25	7.2	P. M.	E
	27	4.1	A. M.	E
	28	1.0	P. M.	E
	29	9.9	"	E

TETHYS.

		h		
Apr.	1	4.7	P. M.	E
	3	2.0	"	E
	5	11.3	A. M.	E
	7	8.5	"	E
	9	5.8	"	E
	11	3.1	"	E
	13	12.4	"	E
	14	9.7	P. M.	E
	16	7.0	"	E
	18	4.3	"	E
	20	1.6	"	E
	22	10.9	"	E
	24	8.2	"	E
	26	5.5	"	E
	28	2.8	"	E
	30	12.1	"	E

DIONE.

		h		
Apr.	2	7.9	P. M.	E
	5	1.6	"	E
	8	7.3	A. M.	E

DIONE Cont.

		h		
Apr.	11	12.9	A. M.	E
	13	6.6	P. M.	E
	16	12.3	"	E
	19	6.0	A. M.	E
	21	11.6	P. M.	E
	24	5.3	"	E
	27	11.0	A. M.	E
	30	4.7	"	E

RHEA.

Apr.	3	12.6	A. M.	E
	7	1.0	P. M.	E
	12	1.4	A. M.	E
	16	1.7	P. M.	E
	21	2.1	A. M.	E
	25	2.5	P. M.	E
	30	2.9	A. M.	E

TITAN.

Apr.	1	5.4	A. M.	I
	5	8.4	"	W
	9	10.3	"	S
	13	5.0	"	E
	17	2.5	"	I
	21	5.5	"	W
	25	7.8	"	S
	29	2.5	"	E

HYPERION.

Apr.	1	4.0	A. M.	E
	7	11.2	"	I
	12	6.2	P. M.	W
	17	3.9	A. M.	S
	22	8.3	"	E
	28	3.9	P. M.	I

IAPETUS.

Apr.	9	9.0	A. M.	E
	27	9.2	"	I

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A.....$0^h\ 52^m\ 32^s$
Decl.....$+81°\ 17'$
Period.....$2^d\ 11^h\ 50^m$

Apr.	2	5 A. M.
	4	4 P. M.
	7	4 A. M.
	9	4 P. M.
	12	4 A. M.
	14	4 P. M.
	17	3 A. M.
	19	3 P. M.
	22	3 A. M.
	24	3 P. M.
	27	3 A. M.
	29	3 P. M.

ALGOL.

R. A.....$3^h\ 1^m\ 1^s$
Decl.....$+40°\ 32'$
Period.....$2^d\ 20^h\ 49^m$

Apr.	3	8 P. M.
	6	5 "
	9	2 "
	12	11 A. M.
	15	8 "
	18	4 "
	21	1 "
	23	10 P. M.
	26	7 "

R CANIS MAJORIS.

R. A.....$7^h\ 14^m\ 30^s$
Decl.....$-16°\ 11'$
Period.....$1^d\ 3^h\ 16^m$

Apr.	1	2 P. M.
	2	5 "
	3	9 "
	4	12 midn.
	6	3 A. M.
	7	6 "
	8	10 "
	9	1 P. M.
	10	4 "
	11	8 "
	12	11 "
	14	2 A. M.
	15	5 "
	16	8 "
	17	12 noon.
	18	3 P. M.
	19	6 "
	20	10 "
	22	1 A. M.
	23	4 "
	24	7 "
	25	11 "
	26	2 P. M.
	27	5 "
	28	8 "
	29	12 midn.

S CANCRI.

R. A.....$8^h\ 37^m\ 39^s$
Decl.....$+19°\ 26'$
Period.....$9^d\ 11^h\ 38^m$

Apr.	3	12 midn.
	13	12 noon.
	23	12 midn.

S ANTLIÆ.

(Every third minimum.)

R. A.....$9^h\ 27^m\ 30^s$
Decl.....$-28°\ 09'$
Period.....$0^d\ 7^h\ 47^m$

Apr.	1	8 P. M.
	2	7 "
	3	7 "
	4	6 "
	5	5 "
	6	4 "
	7	4 "
	8	3 "
	9	2 "
	10	2 "
	11	1 "
	12	12 noon.
	13	12 noon.
	14	10 A. M.
	15	4 "
	16	10 "
	17	9 "
	18	9 "
	19	8 "
	20	7 "
	21	7 "
	22	6 "
	23	5 "
	24	5 "
	25	4 "
	26	3 "
	27	3 "
	28	2 "
	29	1 "
	30	1 "

δ LIBRÆ.

R. A.....$14^h\ 55^m\ 06^s$
Decl.....$-8°\ 05'$
Period.....$2^d 07^h\ 51^m$

Apr.	2	7 P. M.
	5	3 A. M.
	7	11 "
	9	7 P. M.
	12	3 A. M.
	14	10 "
	16	6 P. M.
	19	2 A. M.
	21	10 "
	23	6 P. M.
	26	2 A. M.
	28	10 "
	30	6 P. M.

U CORONÆ.

R. A.....$15^h\ 13^m\ 43^s$
Decl.....$+32°\ 03'$
Period.....$3^d\ 10^h\ 51^m$

Apr.	4	3 A. M.
	7	2 P. M.
	11	1 A. M.
	14	12 noon
	17	10 P. M.
	21	9 A. M.
	24	8 P. M.
	28	7 A. M.

U OPHIUCHI.

R. A.....$17^h\ 10^m\ 56^s$
Decl.....$+1°\ 20'$
Period.....$0^d\ 20^h\ 08^m$

Apr.	1	6 A. M.
	2	2 "
	2	10 P. M.
	3	6 "
	4	1 "
	5	11 A. M.
	6	7 "
	7	3 "
	7	11 P. M.
	8	8 "
	9	4 "
	10	12 noon
	11	8 A. M.
	12	4 "
	12	12 midn.
	13	8 P. M.
	14	4 "
	15	1 "
	16	9 A. M.
	17	5 "
	18	1 "
	18	9 P. M.
	19	5 "
	20	1 "
	21	9 A. M.
	21	6 "
	23	2 "
	23	10 P. M.
	24	6 "
	25	2 "
	26	10 A. M.
	27	6 "
	28	2 "
	28	11 P. M.
	29	7 "
	30	3 "

Y CYGNI.

R. A.....$20^h\ 47^m\ 40^s$
Decl.....$+34°\ 15'$
Period.....$1^d\ 11^h\ 57^m$

Apr.	1	1 P. M.
	3	1 A. M.
	4	1 P. M.

Y CYGNI CONT.		Y CYGNI CONT.		Y CYGNI CONT.	
Apr. 6	1 A. M.	Apr. 15	1 A. M.	Apr. 24	1 A. M.
7	1 P. M.	16	1 P. M.	25	1 P. M.
9	1 A. M.	18	1 A. M.	27	1 A. M.
10	1 P. M.	19	1 P. M.	28	1 P. M.
12	1 A. M.	21	1 A. M.	30	1 A. M.
13	1 P. M.	22	1 P. M.		

Phases and Aspects of the Moon.

	Central Time. d h m
New Moon	Apr. 5 10 00 P. M.
Perigee	" 10 9 40 P. M.
First Quarter	" 12 6 32 P. M.
Full Moon	" 19 9 02 P. M.
Apogee	" 26 1 55 A. M.
Last Quarter	" 27 9 21 P. M.

Occultations Visible at Washington.

Date 1894.	Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle f'm N pt. °	EMERSION Washington M. T. h m	EMERSION Angle f'm N pt. °	Duration. h m
Apr. 8	9 Tauri	7	8 17	74	9 12	266	0 55
10	136 Tauri	5	12 35	160	12 55	209	0 20
12	ω^2 Cancri	6	12 57	65	13 22	333	0 25

Maxima and Minima of Variable Stars.

	MAXIMA		MINIMA	
April	1	R Herculis	5	X Bootis
	2	U Virginis	5	W Scorpii
	10	R Arietis	6	S Cygni
	11	V Leonis	13	R Canum Ven.
	12	X Scorpii	15	U Monocerotis
	14	V Capricorni	15	U Bootis
	16	L^2 Puppis	18	S Ursæ Maj.
	18	V Capricorni	19	R Lyræ
	20	U Capricorni	23	R Ursæ Maj.
			23	S Vulpeculæ
			25	S Piscium
			28	T Arietis

New Asteroids 1894 AQ, AR and AS.—These were discovered photographically by Charlois at Nice on the nights of Jan. 8 and 10. Their positions were as follows:

		G. M. T. h m	R. A. h m s	Decl. °	Daily motion ′ ′	Mag.
1894	AQ Jan. 8	9 25	7 21 48	+ 20 01	− 48 + 1	13
	AR 8	9 25	7 36 12	+ 24 10	− 60 + 4	13
	AS 10	10 45	8 33 16	+ 17 48	− 44 + 6	12.5

Numeration of the Asteroids of 1893.—Dr. A. Berberich who prepares the tables of Minor Planets for the *Berliner Jahrbuch* sends us the following note:

"You gave recently the numeration of the Minor Planets from *Astronomische Nachrichten* 3194. You will have seen, by *Bulletin Astronomique* Nov. and *Astr. Nach.* 3201, that No. 359 must be omitted, as 1893 L proves to be identi-

cal with (89) Julia. I took every precaution in order to avoid similar errors in computing the dates of oppositions for 1894, but it is impossible to vouch for the exactness of all of 240 ephemerides, only 60 of which are computed in duplicate.

"The later planets of 1893 receive the following numbers.

		Discovered by	
1893	AJ = 373	Charlois	Sept. 15
	AK = 374	"	Sept. 18
	AL = 375	"	Sept. 18
	AM = 376	"	Sept. 18
	AN = 377	"	Sept. 20
	AP = 378	"	Dec. 6

"The name 'Chicago,' chosen at the astronomical Congress for a minor planet, has been conferred by Professor M. Wolf upon the planet (334). This planet presents a good means of determining the mass of Jupiter. It undergoes very remarkable perturbations by that great planet. The planets (153) Hilda, (190) Ismene, (279) Thule and (361) are also exposed to considerable perturbations, but only in future times. A conjunction of Hilda and Jupiter some years ago coincided with the perihelion passage of Hilda so that the distance of the two bodies has been at a maximum.

"I find that the Watson planet (175) Andromache, rediscovered in 1893 by Charlois and photographed in 1892 by Wolf, was in proximity to Jupiter in 1886-87, being near aphelion and moving very slowly. As the period of (175) is nearly one-half of that of Jupiter that conjunction will be repeated every 11–12 years, in about the same position. The effect will be a retardation of the mean motion, probably of large amount. I think the moderately distant planets are of greater value for the determination of Jupiter's mass than those very remote, like Hilda, since the conjunctions with Jupiter are much more frequent. Thus in one century we may observe 8 conjunctions of Andromache but only 4 of Hilda, and scarcely 3 of Thule, with Jupiter."

Ephemeris of Comet *c* 1893 (Brooks).—From Dr. Krueger's elements I have computed the following ephemeris of Comet Brooks.

G. *M.* T. 1894	App. R. A. h m s	App. Dec. ° ′	Log *r*	Log *Δ*
Mar. 1.5	0 57 6	+ 55 19	0.4318	0.4648
2.5	0 58 48	55 11		
3.5	1 0 30	55 3		
4.5	1 2 10	54 54		
5.5	1 3 49	54 46	0.4398	0.4795
6.5	1 5 26	54 38		
7.5	1 7 3	54 31		
8.5	1 8 39	54 24		
9.5	1 10 14	54 17	0.4477	0.4936
10.5	1 11 47	54 11		
11.5	1 13 20	54 6		
12.5	1 14 53	54 0		
13.5	1 16 26	53 54	0.4551	0.5066
14.5	1 18 2	53 50		
15.5	1 19 37	53 46		
16.5	1 21 8	53 42		
17.5	1 22 38	53 38	0.4600	0.5168
18.5	1 24 2	53 29		
19.5	1 25 22	53 21		
20.5	1 26 44	53 13		

G. M. T. 1894	App. R. A. h m s	App. Dec. ° ′	Log *r*	Log *Δ*
21.5	1 28 6	53 5	0.4695	0.5290
22.5	1 29 24	52 55		
23.5	1 30 43	52 42		
24.5	1 32 11	52 31		
25.5	1 33 38	52 19	0.4765	0.5348
26.5	1 35 0	52 9		
27.5	1 36 21	51 58		
28.5	1 37 43	51 48		
29.5	1 39 2	51 37	0.4834	0.5403
30.5	1 40 21	51 27		
31.5	1 41 39	+ 51 17		

O. C. WENDELL.

Harvard College Observatory, Feb. 14, 1894.

Brilliant Meteor.—A large ball of fire supposed to be a meteor was seen here February 16th, 7:45 P. M., standard time. The strange sight was witnessed by a great many people all agreeing that the meteor appeared to start in the western heavens, near the horizon and traveled in a northwesterly direction until it disappeared.

Some observers claim that the ball after travelling some distance broke, one piece going toward the ground, the other continuing in a straight course for several seconds. I was not fortunate enough myself to witness the display, consequently cannot vouch for the direction and duration of flight, but must be content with the testimony of others.

CHAS. E. MYERS.

Canton, Ohio, Feb. 17th, 1894.

NEWS AND NOTES.

Dr. See's Researches on the Orbit of α Centauri.—The *Monthly Notices* of the Royal Astronomical Society for December contains a new and important investigation of the orbit of α Centauri by Dr. T. J. J. See, who has collected from original sources all the observations from the earliest times to the present date, and derived new elements which ought to give the motion of this celestial binary with great precision. The author gives all the measures from 1690 to 1893.50 with the number of nights on which they were made, and then forms means which are employed as the basis of the apparent orbit, from which the elements are found by the method of Klinkerfues. Dr. See proceeds by the well known graphical method, and gets the following:

ELEMENTS OF α CENTAURI.

$P = 81.07$ years.
$T = 1876.62$
$e = 0.52$
$\Omega = 25°.45$ (1900.0)
$i = 79°.74$
$\lambda = 51°.56$
$a = 17''.705$
$n = +4°.44184.$

APPARENT ORBIT.

Length of major axis	$= 32''.50$
Length of minor axis	$= 6\ .16$
Angle of major axis	$= 27°.2$
Angle of periastron	$= 38\ .0$
Distance of star from centre	$= 5''.94$

A beautiful illustration of the apparent orbit accompanies the paper, and the author also gives a comparison of the computed and observed places, which shows a very good agreement. Dr. See finds by means of his elements and the parallax of Gill and Elkin ($0''.75$) that the semi-major axis of the orbit of α Centauri is 23.592 astronomical units; so that the companion moves in an orbit which is about a mean between those of the planets Uranus and Neptune, but the eccentricity is so high that in periastron the distance (11.3) but little surpasses that of Saturn, while in apastron it considerably surpasses the distance of Neptune from the Sun, becoming 36.0 astronomical units. The author finds the combined mass of the two components of α Centauri to be 1.998 times the mass of the Sun and Earth, and calls attention to the desirability of determining the relative masses of the two components, for the light the result would throw upon the cosmogony of double-stars.

Dr. See expresses his warm thanks to the observers in the Southern hemisphere —to Messrs. Tebbutt, Pickering, Douglass, Russell, Sellors, Gill and Finlay—for securing sets of measures expressly for the present research: also to Herr Lundendorff of Berlin, and to Professor Burnham and Mr. Foley of Chicago for kindly assistance in the investigation. In conclusion he refers to the recent elements by Mr. A. W. Roberts in the *Astronomische Nachrichten*, No. 3175, which are:

T	$= 1875.715$
P	$= 81.185$ years
e	$= 0.52865$
λ	$= 52°\ \ 0'\ 58''$
i	$= 79\ \ 21\ \ 36$
ω	$= 25\ \ \ 5\ \ 50$ (1900)
a	$= 17''.71$

It is thus seen that Dr. See and Mr. Roberts, working independently, by different methods, have arrived at elements which are very nearly the same, and hence there can hardly be a doubt but that the orbit of α Centauri, which has been hitherto uncertain (the periods obtained ranging from 77 to 88 years), is now the best determined of any known double star. If any correction to Dr. See's elements should in the course of time be found desirable, it seems that they would of necessity be very small.

The Solar Image Reflected in the Seas of Mars.—M. Flammarion (La Planete Mars, p. 220) speaks of the possibility of seeing the image of the Sun reflected in the Seas of Mars. He estimates that in favorable circumstances it might appear like a star of the 3rd magnitude, but he adds that this implies that the surface of the sea should be as calm as a mirror. I have gone over M. Flammarion's calculation. It seems to me that the allowance he has made for absorption of light in its double passage through the Martian atmosphere, is probably insufficient, and that it would more likely be of the 4th magnitude.

What he says with regard to the calmness of the sea, is of course true, but the area of sea concerned in forming the image would only be a circle of about 7 miles diameter; and remembering that the winds of Mars are probably much weaker than ours, it does not seem so unlikely that this extent may be sometimes calm.

It seems to me that the reason this image has not hitherto been observed, is not improbably because it has not been systematically and properly looked for. We must remember that at a given instant it can only be formed at a particular point of the disc, and if at this instant this point is occupied by a portion of the surface incapable of specular reflection, no image can be formed.

Now only a very small portion of the surface appears to be so capable. The land, of course, is not; and if Professor Pickering is right, the majority of the so-called seas are land. I think it is more likely that the areas of greenish tint, generally called sea, but regarded as land by Professor Pickering, are really sea, but very shallow and choked up with rocks and small islands. In this case also they would be unfit to form an image.

So, I think, we could only expect it in the two areas regarded as sea by Professor Pickering, viz., Herschel II Strait, and part of the Hour-glass Sea.

The time when an image could be expected in these seas could be easily calculated beforehand, and it might then be looked for.

To prevent the brilliancy of the rest of the disc from overpowering the light, I would suggest that a diaphragm with a small hole in it be employed.

Obviously the point of the disc where it should be formed, is exactly midway between the centre of the disc and the centre of the illuminated hemisphere. In itself, this observation is not very important, but it would definitely settle the question whether the bright or dark markings are seas.

Of course, if the sea was *perfectly* calm, this image should be small, regular and starlike. And, in that case, it could hardly have escaped notice, even if considerably below the 4th magnitude; but evidently *perfect* calmness must be unusual. With waves following one another regularly, it would be elongated into a streak, little more than 7 miles wide, and 100 or more miles long. With irregular ripples, it would appear nebulous, but even then it might be visible if properly looked for.

I have made a rough calculation, with the result that I doubt if this can be effectively tested at the next opposition. Throughout this, the Earth, as seen from Mars, will have a high southerly declination, and the Sun being far south, also, the spot on the disc where the image could be formed will be much too far south for either of the seas in question. It is not till the Sun nears the equator, that it can fall even in Herschel II Strait, and this will not be till nearly the end of January, when the planet's distance from us will be considerable, and the image if formed, correspondingly feeble.

During the two months before and after the opposition, the locus will traverse a belt crossing Maraldi, Hooke and Terby Seas.

It might, perhaps, be worth while to look for the reflection in those Seas, on the chance that they *may* be water.

J. R. HOLT.

6 Harrington St., Dublin, Ireland.

The Constant of Aberration.—In Bulletin No. 28 of the U. S. C. and G. Survey, Mr. Preston gives a detailed discussion of the observations made at Waikiki, H. I., by which a correction is sought for the commonly accepted values of the constant of aberration (20″.445). Mr. Preston says in closing that the defini-

tive result of the constant of aberration from the latitude observations of 1891-1892, made at Waikiki, Hawaiian Islands, on the part of the United States Coast and Geodetic Survey, is therefore

$$\text{Constant of aberration} = 20''.433 \pm 0''.034.$$

This value of the aberration constant, combined with the latest determinations of the velocity of light (V = 186,330 miles) and Clarke's value for the Earth's radius (R = 3963·30 miles), gives the Sun's distance and equatorial horizontal parallax as follows:

$$\text{Distance} = 92{,}709{,}000 \text{ miles.}$$
$$\text{Parallax} = 8''.82.$$

H. A. N. *in March Am. Jour. Science.*

The Dawn of Astronomy.—Mr. J. Norman Lockyer has just written a book entitled, *The Dawn of Astronomy*. It is published by Messrs. Macmillan & Company, London and New York, in neat and attractive form, with large page, clean type, wide margin, very heavy paper, 121 fine illustrations and a contents of 425 pages.

During the last three years the author has been industriously gathering facts and making a study of them that pertain especially to the temple worship and the mythology of the early Egyptians, in order to learn as much as possible about their astronomical views. He was led to this study, by an incident while on a visit to the ruins of the Parthenon in 1890. The curious direction in which the Parthenon was built and also the many changes in direction in the foundations at Eleusis revealed by French excavations were facts that awakened interest and suggested the query, whether or not, the choice of these various directions in which ancient temples should face, had its origin in important astronomical facts. If so what were the directions and what the celestial objects to which they were dedicated?

Upon inquiry it was learned that but scanty information could be found to answer such questions in relation to the ancient temples, for scarcely anything of the kind was known even in regard to churches outside of England and Germany.

However, later it was learned that Professor Nissen of Germany had published some papers on the orientation of the ancient Egyptian temples, and these with needed information given by archeological friends opened the way for future work of an important kind. These ancient temple sites should be accurately surveyed, astronomers should furnish tables of the places of many stars reaching back 7,000 B. C., and the Egyptologist should read inscriptions for suggestions. In these three different lines of study there seemed to be in the outset much of promise and considerable has already been realized as is shown in the contents of this book.

The author begins the first chapter with the worship of the Sun and the Dawn by the first civilizations which archeologists place in the Nile valley and adjacent countries in western Asia. Two other civilizations of later time and different type were India and China, with paper records but no monuments of high antiquity, for, on the authority of Max Müller we must believe that the known temples of India are relatively of modern origin.

If we can go back in China's and India's history 4000 years and by Babylonian tablets 5000 years, those of Egypt, as estimated by various authors, will carry us back 6000 or 7000 years. So it is well, if possible, to get first glimpses of Egyptian Astronomy and these are shown by the Rosetta Stone, the temples of Edfû and Denderah with its circular zodiac, and the Egyptian Pantheon and

the tablet of kings at Abydos, and through these and numerous others like them to learn of the early worship and the study of the stars accompanying it. Chapters four, five and six explain, in popular way, elemental ideas of astronomy, and chapter seven speaks of the methods of determining the orientation of temples, giving illustrations of instruments used, maps of magnetic variation. The earliest solar shrines of Egypt are fully illustrated and the principal ones are those at Abydos, Ann, Collossi on the plains of Thebes, Gizeh and one near the Sphinx. Other similar shrines elsewhere are also described, such as the Pekin Sun temple, the temple at Jerusalem, St. Peter's at Rome and that at Karnak.

Next is considered the rising and setting of the stars, the Egyptian heavens, and more fully the zodiacs of Denderah, the circumpolar constellations and the myth of Horus.

In the fifteenth chapter the temples dedicated to the stars are presented, and further inquiries made concerning them and the inscriptions found in connection with the ruins, followed by a special study made of the star temple at Karnak. Later chapters of special interest are those concerning the Egyptian year and the Nile, the Euphrates gods and the Nile, the vague and Syrian years, the calendar and its revision, the fixed year and festival calendars, the mythology of Isis and Osiris, the early temple and great pyramid builders, north and south star temples, the origin of Egyptian astronomy under the name of the northern schools, and also under the Thebes school, general conclusions as to the north and south races, the Egyptian and Babylonian ecliptic constellations, and the influence of Egypt upon temple orientation in Greece.

This important book opens a new field for work in the early history of astronomy.

Photographic Chart of the Sky.—Vol. II, part II, of the "*Bulletin du Comite international permanent*," contains important memiors by Messrs. Kapteyn and Loewy, the former on "Systematic difference between the photographic and visual magnitudes of stars in different regions of the sky," the latter a second paper on "Construction of the catalogue based upon the plates for the chart of the sky." There is also a description by Prosper Henry of the method of measurement and reduction of photographs at the Paris Observatory.

In the "correspondence" we notice several complaints in regard to lack of permanence in the *reseau*. It is liable to scratches and pin-holes in a very short time. Several observers suggested that the réseau be omitted from the plates for the chart although it should be retained upon the plates for the catalogue.

As to the progress of the work, the following report is given of the number of plates which have beeh obtained for the catalogue and for the chart:

	For Catalogue.	For Chart.
Paris	397	137
M. Baillaud	140	173
" Rayet	164	21
" Trépied	570	64
" Denza	49	20
" Russell	300	
" Donner	120	

H. C. W.

The Rings of Saturn.—It is not often that we hear of a woman receiving the degree of Doctor of Sciences, especially in the line of mathematics. That degree has recently been conferred by the University of Paris upon Miss Dorothea

Klumpke, a young American woman, of San Francisco, who a few years ago obtained admission to the Observatory of Paris, and has since been engaged there in study and astronomical investigation. She was for a time in charge of the great reflector, but is now in charge of the Bureau of Measurements, which has for its work the measurement of the positions of stars upon the photographic plates, which are being taken for the purpose of making a catalogue of all stars down to the 11th magnitude.

For her thesis Miss Klumpke chose the subject of the form of Saturn's rings. She gives a brief history of the discovery of the rings, theories as to their constitution and study of their equilibrium. She herself takes up the problem of the figure of a fluid ring, or a solid ring covered with fluid, in equilibrium around Saturn. This problem was first treated by La Place, later by Mdme. Kowalewski and M. Tisserand. Mdme Kowalewski considered the figure of the ring to be generated by the revolution of a plane curve about an axis passing through the center of Saturn and perpendicular to the plane of the curve's motion. Miss Klumpke carries on her investigation by including terms of a higher order and shows that their effect is very slight. She obtains for the equations of the cross-section of the ring

$$y = -\cos t, \quad x = 0.385 \sin t - 0.188\ \sigma\ \sin 2t$$

in which t is a variable parameter and σ is the ratio of the semi-major axis of the cross-section to the distance of its center from the centre of Saturn.

In the second part she investigates the simpler hypothetical case, in which the mass of Saturn is considered to be zero, the rings being subject only to the centrifrugal force of its motion and the mutual attraction of its molecules. A first approximation including only the second powers of σ gives the figure of cross-section of the ring as a circle. Including terms in σ^3 the figure becomes an ellipse, and when the terms involving σ^4 are considered the cross-section becomes egg-shaped with the small end inward. H. C. W.

Bibliography of Astronomy.—Works published during January, 1894. Compiled by W. Wesley and Son, 28 Essex Street, Strand, London, England, who will also supply any of the works, if desired.

Fabry (L.) Etudé sur la probabilité des comètes hyperboliques et l'origine des comètes. 4to. Marseille, 1894. $1.25.

Guyon (E.) et H. Willotte. Cours elementaire d'astronomie, with 170 engravings and 2 plates. 8vo. Paris, 1894. $2.25.

Knight (G.) A short history of astronomy. 16mo. 1894. 15 cts.

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Mayer (I). Sternverzeichniss. Nach den Beobachtungen auf der Goettinger Sternwarte, 1756-60. Neu bearbeitet von A. Auwers. 4to. Leipzig, 1894, $5.75.

Poynting (J. H.) The mean density of the Earth, 7 plates and engravings. 8vo., cloth. 1894. $3.15.

Proctor (R. A). The expanse of Heaven: a series of essays on the wonders of the firmament. New edition, post 8vo., cloth. 1894. 90 cts.

Astronomical and Physical Society of Toronto, Canada.—Meeting of Jan. 23, 1894; chair, Dr. Larratt W. Smith, Q. C., vice president. He thanked the members for his recent re-election.

Mr. C. H. Collison, B. A., of Upper Canada College, was elected an active member. Honorary membership was conferred upon Sir Robert S. Ball, LL. D., Lowndean Professor of Astronomy, King's College, Cambridge, and director of the University Observatory; upon Geo. H. Darwin, M. A., Plumian Professor of Astronomy, Trinity College, Cambridge; upon Professor H. C. Vogel, director of the Astro-Physical Observatory, Potsdam, Germany. Professors C. H. McLeod, Ma. E., etc., Director of McGill College Observatory, Montreal; W. A. Rogers, Colby University, Waterville, Me.; Edward W. Maunder, F. R. A. S., First-Class Physical Assistant, Greenwich Observatory; Rev. T. E. Espin, F. R. A. S., Director of Tow Law Observatory, Darlington, Eng., were elected corresponding members.

Librarian G. G. Pursey reported receipt of publications, including those of the Royal Society and one presented by Mr. A. Aaronsberg, Toronto.

Observations on the Sun, on the planets and on the zodiacal light were reported by several members. Dr. A. D. Watson referred to Saturn as being especially beautiful, his rings having opened out farther than has been observable for years. With but a three-inch telescope, he had seen Cassini's division.

Most of the evening was occupied reading and discussing these five-minute papers: "Jupiter's Atmosphere," Mr. R. B. Ellis; "The distances of Jupiter's Satellites" (with diagram), Mr. A. Elvins; "The Ellipticity of the Orbits of Jupiter's Satellites," Mr. A. Harvey; "Certain Phenomena Connected with Satellites I and II," Dr. A. D. Watson; "The Mythology of Jupiter," Mr. W. B. Musson; "The Density of Jupiter," Mr. T. Lindsay. None of the papers presumed to be original, but all were clever.

Meeting of Feb. 6. Mr. Arthur Harvey presided. Honorary membership conferred upon M. Otto Struve of St. Petersburg; corresponding membership conferred upon M. Paul Henry, elder brother of the Henry brothers of Paris Observatory, France.

Upon the subject of changing astronomical time to begin at midnight instead of at noon, a letter was read from Mr. Sanford Fleming, C. M. G., recently returned to Ottawa after a 24,000-miles trip on the sea. He had conferred with captains and other officers, and all were agreed that the change should be made. In answer to the circulars sent out there had been received two hundred answers, 152 of which were for the change and 48 in opposition to it.

Mr. Andrew Elvins reported some excellent observations of Venus. She showed a thin crescent, gapped along the terminator. Mr. Elvins had also seen the belts of Jupiter very plainly. It was stated that an observer in Barbados had seen the dark portion of the disc of Venus, similar to the phenomenon which appears at a new moon.

Mr. Joseph W. Allen, father of Grant Allen, the famous novelist and scientist, read the paper of the evening, which was a defence of his son's book, "Force and Energy."

JOHN A. COPLAND.

The Chicago Academy of Sciences, Section of Mathematics and Astronomy, Feb. 6th.—Professor G. W. Hough, President, in the chair. The Recorder announced that there was no business before the Section requiring special action, and the meeting at once proceeded to the program of the the evening. Professor G. W. Hough read the first paper on the "*Evolution of the telescope.*" In tracing the

history of lenses from Roman times to the present date, the speaker remarked that the earliest mention of anything that could be called a lens, is that of a "burning sphere" by Aristophanes, 424 B. C., and he considered the next record that of the astronomer Cleomedes who observed that a bottle magnifies objects seen through it. For the next 1000 years very little was done in optics, but the Arabian astronomers had given some attention to the subject about the time of Ibn Jounis, 1000 A. D.

In modern times the study of lenses begins with Roger Bacon, who used them for spectacles. When lenses were in general use for aiding the vision, of course a very slight step would give a magnifying glass. In 1608 Lippershey, the Dutch spectacle-maker, constructed a telescope which was applied to terrestrial objects; and Galileo soon afterwards invented the astronomical telescope, which created an epoch in the history of science. The speaker gave an interesting account of Galileo's observations of the mountains on the Moon, the spots on the Sun, the Satellites of Jupiter, and the Rings of Saturn; and after sketching the history of the "long" telescopes of the next century, and describing the method of using a telescope without a tube, he came down to the reflector invented by Gregory in 1663, and preferred by Newton on account of the imperfect definition of refractors arising from chromatic aberration. Professor Hough pointed out the importance of Dolland's services in using two kinds of glass with different refractive indices, and called attention to the problem of securing large homogeneous pieces of glass, which was finally solved by Frauenhofer, but which long retarded the use of the refractor.

The speaker then projected on the screen illustrations of most of the celebrated modern telescopes, beginning with the great reflector of Sir Wm. Herschel and concluding with the Yerkes 40-inch refractor. In reviewing recent progress in telescope-making the speaker pointed out the unexampled services of Alvan Clark & Sons, whose first large telescope was the 18-inch Dearborn refractor at Evanston, with which he and Professor Burnham have discovered and measured so many close double stars. In conclusion Professor Hough remarked that refractors were preferred by American astronomers, but that for some purposes the reflectors have their advantages, and they are still used in England and France. A general discussion followed, in which all the observers present took part.

Dr. Henry Crew of Northwestern University read the second paper of the evening on "*Refraction at the Eye-end of the telescope.*" Professor Crew defined the telescope as an instrumeut for transforming a plane wave surface into a spherical one. He then interpreted Airy's equation for the distribution of light in the field; and pointed out the limit which the finite value of the light waves sets to the defining power of any aperture. Various methods were shown for improving the definition; but in all cases a practical difficulty was encountered in loss of light.

Attention was called to Professor Michelson's extended definition of "defining power" and to some of the possibilities of the refractometer in astronomical work. At the close of the session a number of diffraction patterns resulting from apertures of various forms were shown by means of a small telescope and an artificial star. Professor Burnham, Dr. See and several others took part in the discussion of Dr. Crew's interesting paper, after which the Section adjourned.

T. J. J. See, Recorder.

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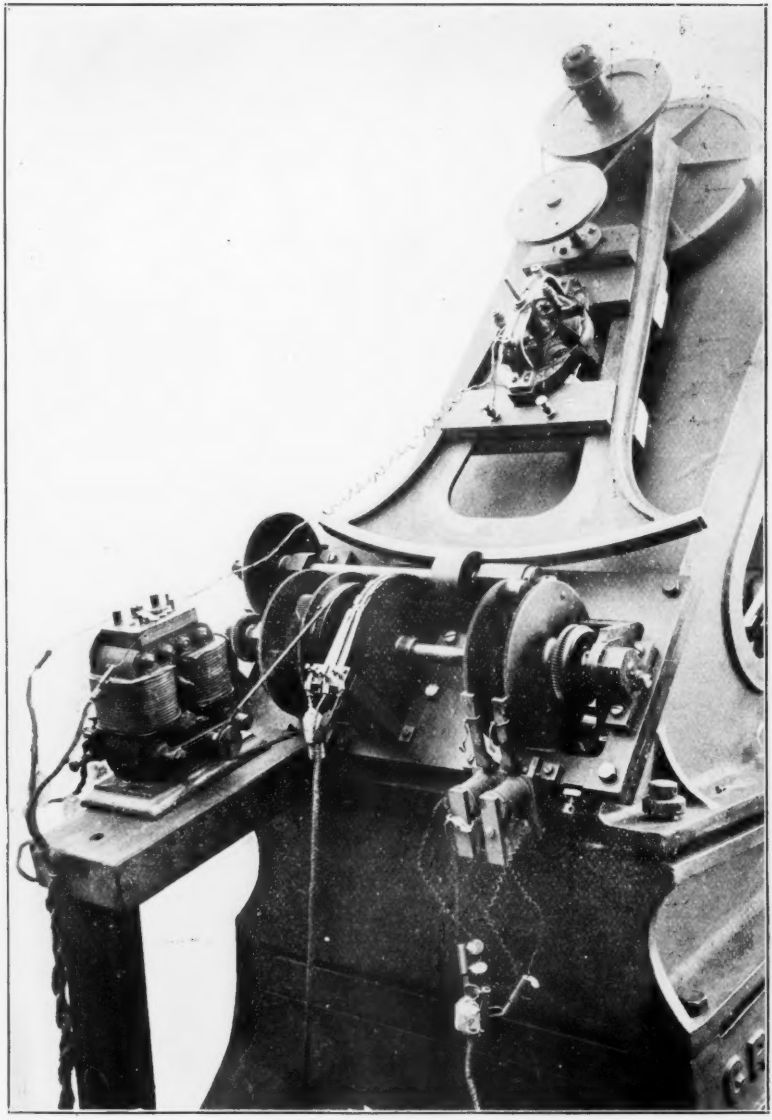

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